ADDRESS

DELIVERED AT

THE ANNIVERSARY MEETING

OF THE

GEOLOGICAL SOCIETY OF LONDON,

On the 18*th of* FEBRUARY, 1842;

AND

THE ANNOUNCEMENT OF THE AWARD

OF THE

WOLLASTON MEDAL AND DONATION FUND

FOR THE SAME YEAR.

BY RODERICK IMPEY MURCHISON, F.R.S.,

PRESIDENT OF THE SOCIETY.

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AWARD

OF THE

WOLLASTON MEDAL AND DONATION FUND

FOR 1842.

[From the Proceedings of the Geological Society, Feb. 1842.]

OBSERVATIONS BY THE PRESIDENT.

THE Wollaston Medal has been this year awarded to M. Leopold von Buch, for "the eminent services he has rendered to Geology by his extraordinary and unremitting exertions during a long series of years, and for his recent researches in Palæontology."

Since geology has been a science no individual has more successfully applied a powerful mind to its cultivation, or more liberally expended his private means in advancing its progress than Leopold von Buch. The chief works by which his fame was reared are well known; but with the numberless memoirs printed and published at his own charge and gratuitously distributed, I regret to say, English geologists are by no means sufficiently acquainted; and justice cannot be rendered to him until the whole of his researches are brought before the public in a combined form. In the mean time we offer our Medal to this distinguished man, to show that we seek to reward him not only for his acknowledged great works, but also for those efforts to advance science, which are too little known. Such, for instance, is the large geological map of Germany, including the Alps and adjacent regions, published without allusion to his name, and commonly known as the map of Martin Schropp and Co.; a most remarkable production, whether we consider the date of its publication or the expenditure of mind, labour, and money which it must have cost the author. And although the result of these labours has since been improved upon by the efforts of several of his countrymen, among whom the names of Hoffmann and Von Dechen stand prominently forward, it is well to know that no one has more untiringly contributed new information to his younger friends than Von Buch. When a traveller at Berlin, upwards of two years ago,

and lost in admiration at the progress which physical geography and geological maps were making in that metropolis, I was much surprised to learn, that M. von Buch had in his possession an unpublished geological map of Bohemia, all, be it observed, worked out by his own patient observations on foot. Aware, from a former rapid survey of that country, that our knowledge of Bohemia was still very imperfect, I obtained from the author a coloured copy, which I first exhibited to the British Association at Glasgow (1840), and which I now present to the Geological Society.

Again, after successfully developing, in the spirit of a true philosopher, the recondite phænomena of the metamorphism of rocks by the most laborious pedestrian efforts, have we not seen, that as years rolled on and our veteran leader began to feel, that the toil of gaining the mountain crest must soon pass from his own limbs to those of younger men, he has so vigorously applied his mind to Palæontology as to throw new lights over this department of our many-headed science? No sooner did he grapple with this task, and that too when he had passed the meridian of life, than he displayed the same originality of mind which had marked all his previous inquiries. Subjecting the family of *Ammonites* to revision, and convinced that their innumerable species were not founded on true natural distinctions, he took the lines of suture as a basis, and thereon established a limited number of normal or typical forms, each characteristic of certain strata. The *Terebratulæ*, so common in all the secondary strata, were next passed in review, and types were fixed upon, to which a number of slightly varying forms were referred, a work which our French brethren have considered so important, that they have republished it in the Transactions of the Geological Society of France. Then followed his illustration of the fossils of South America, collected by his great countryman Humboldt. Whilst I merely enumerate these works, I may be allowed to say a few words respecting his last published volume, "On the Fossils of Russia," because, together with my associates, M. de Verneuil and Count Keyserling, I have had the means of forming an opinion of its value. Simply furnished with collections of organic remains from various parts of the Russian empire, M. de Buch, without ever visiting the country, assigned to each form he examined its position in the geological series. As the researches of my

terially assisted in the great advance which geology has made, and recent publications have proved that his love of science was as ardent as ever, and that the importance of the labours in which he was engaged was undiminished.

In assigning the donation of the Wollaston Fund to Mr. Morris, the President thus expressed himself:—

Mr. Morris,

The Council o. the Geological Society have awarded to you the proceeds of the Wollaston Fund during the past year, to assist you in preparing for publication a table of British Organic Remains, in which you have been for some time engaged, and which, from the specimens laid before us, we believe will be of very great service in promoting the accurate study of Geology. The value of the table of the late Mr. Woodward has been acknowledged; but his premature death having prevented him from enlarging its sphere as our science advanced, a new and much more comprehensive work has been urgently demanded. I am happy that the task of meeting our wants has been undertaken by one well qualified, like yourself, by diligent research and a competent acquaintance with Natural History; whilst in thus consulting your own wishes, the Council of the Geological Society is persuaded that they are acting in the very spirit of Wollaston's bequest, not treasuring up money parsimoniously, but expending it liberally upon the very fitting occasion which your ability and research have called forth.

To which Mr. Morris replied,—

Sir,—I cannot sufficiently express my grateful thanks to the Society for the unexpected compliment that has this day been conferred upon me, and more especially for the disinterested feeling which has actuated the Council in awarding to one almost unknown to them, this honourable testimonial of their approbation, and not the less flattering to me, Sir, your kind and courteous manner in communicating the same; and I trust that my efforts for the promotion of geological science, which have already entailed upon me so many obligations to various members of the Society, may still excite their willing co-operation towards perfecting a catalogue of British Fossil Remains, from which the geologist may reason with confidence, and which the naturalist may consult with advantage.

ADDRESS

TO THE

GEOLOGICAL SOCIETY,

Delivered at the Anniversary, on the 18th of February, 1842.

BY RODERICK IMPEY MURCHISON, F.R.S.,

PRESIDENT OF THE SOCIETY.

GENTLEMEN,

ALTHOUGH acquainted with my intended absence from this country during many months of the past year, you nevertheless honoured me with the station which I occupy, kindly intimating that the active pursuits of geology should not be a bar to the enjoyment of the chief distinction which you can confer. In thanking you sincerely for that proof of your good opinion, permit me to say, that if the presiding over a body of gentlemen so well knit together for a common purpose, were all that you expected from me, light as well as agreeable would be the task. A charge, however, of a more serious nature is the composition of an anniversary discourse, in which I am expected to treat of the progress of geology during the past year. So very expanded is the present condition of our science, that he who attempts to give a clear synopsis of all that has been done in different parts of the globe, even in one year, and to indicate the desiderata to be accomplished, must make himself master of numerous foreign works. An active observer cannot well execute such a task. On the other hand, if your President should simply review the last year's proceedings of our own Society, he will but poorly serve you, for our abstracts make you well acquainted with the prominent facts and opinions of the authors. I will, therefore, adopt a middle course, and without attempting a complete sketch of the progress of geology, or tiring you with a dry analysis of our performances, permit me to select for your consideration what I consider to be the chief subjects of present geological interest, whether

foreign or British, and so class them that their bearing upon the advance of our science may at once be seized. If in so doing I should fail to illustrate points which some of you may consider to be better suited to this address than those which I bring before you, I trust you will recollect how brief is the season during which I have been able to detach myself from my own line of inquiry, and how imperfectly, therefore, I have been able to study the works of my contemporaries.

OBITUARY.

Before we enter upon the consideration of the progress of geology, let us pay our homage to the memory of those deceased Fellows who laboured to promote our science.

On this occasion we have to mourn over one whose genius has won for himself an imperishable name. By the purest feeling of the beauties of nature, by the manly simplicity of his character, and by his sterling good sense, CHANTREY was led to his peculiar excellences as an artist. Admiring him for his unrivalled excellence in art, we geologists loved him also for the endearing qualities of the man.

Sir Francis Chantrey was a member of our Council, a frequent attendant both at our social meetings and in the rooms of the Society, and on all occasions was happy to serve us, though invariably on one condition, that he was never put prominently forward. If then his presence has often debarred us from expressing in his hearing those sentiments of esteem with which he inspired us, we have this day, alas, the opportunity of giving full utterance to our sorrow. Even as working geologists his memory has claims upon us in more than one department of our own science. Lest his biographers should not glean the facts, I must now state that we have benefited by his sound advice concerning the application of colours to our geological maps, and on the best means for preserving organic remains, which presented difficulties from their size, their condition, or the nature of the rock in which they were imbedded; and upon several occasions he has assisted us by superintending the moulding of osteological specimens which have been brought to this country, and of which it was important to obtain casts. Indeed at all times was his assistance freely given where it could be useful,

and his chisel even has been employed in dissecting from their matrix the bones of fossil reptiles.

Snatched from us in the zenith of his bright career, the strong bias of his mind shines forth in his splendid bequest to the Royal Academy. Persuaded (from whatever cause arising) that art is not appropriately encouraged in our country, he has decreed that British genius shall no longer droop for want of enlightened assistance. His munificent endowment of native art is Chantrey's proudest monument, and must, indeed, produce effects far beyond the portals of our national gallery. But whatever may be the ultimate effect of this patriotic bequest, we must gratefully admire the spirit which dictated it, and ever feel a just pride in having had so good a man for our warm friend, so great a sculptor for a co-operating associate.

Mr. BOWMAN, whom we have very recently lost, was a naturalist who, as far as his other avocations permitted, did much good service in practical geology. His chief attainments lay in botany, and he is the author of several publications upon that science. Residing formerly at Wrexham, he acquired a very intimate knowledge of the carboniferous tracts to the south and west of that town, and he communicated to myself a good deal of valuable, original matter connected with them and the adjacent older rocks, shortly before the 'Silurian System' appeared. He afterwards favoured this Society with some very excellent details concerning a group of Upper Silurian rocks in Denbighshire, and their junction with old red sandstone and mountain limestone, pointing out some essential mineral variations in these rocks upon the northern frontier of Wales, as compared with the typical strata of the same age in Shropshire, and the centre of the Silurian region. After he removed his residence to Manchester, where he died, he pursued science with renewed zeal, and was one of the most active promoters and officers of the Geological Society of that town. To be convinced indeed of his ardour and research, you have only to refer to the first volume of the Transactions of the Manchester Society, and you will find that four out of eleven memoirs are from the pen of our late associate. I shall also have occasion in the sequel to advert to a short memoir upon the glacial question which is amongst his most recent productions*. His loss in Manchester must indeed be seriously

* Philosophical Magazine, November, 1841.

felt, and from my own knowledge I can state that his absence is not only to be regretted in these rooms, but also that his presence will be much missed in the approaching assembly of the British Association for the Advancement of Science, for he had been a frequent attendant at former meetings, and had never failed both to communicate papers and to serve in any office in which he could be useful. In estimating his character, I should say that Mr. Bowman took a high place in that class of authors who silently but steadily advance science by short and clear monographs on subjects with which they are familiar. As the class is not large, so can we ill afford to spare the assistance of one who, like Mr. Bowman, really distinguished himself in this modest but highly useful walk.

By the death of Mr. Thomas Edington of Glasgow, we lose one of the old and valued members of the Society, and whose name is honourably associated with that of the early school of Scottish mineralogists. Every geologist who has had occasion to visit the West of Scotland, found in his house a hearty welcome, and in his beautiful museum much instruction respecting the vast variety of simple minerals in which that region abounds. At the meeting of the British Association for the Advancement of Science, held at Glasgow, he filled the office of one of the local secretaries, on which occasion he was untiring in his exertions, and unbounded in his hospitality, whilst he was of signal use in cementing the bonds of kind feeling between his countrymen and the men of science who came among them as visitors. Having been informed that Mr. Edington's minerals must be disposed of, I beg to express my hope that a collection so choice, and so highly esteemed by mineralogists, may find some enlightened purchaser worthy of its contents.

Among our other deceased Fellows, I have still to mention three whose names are connected with our pursuits, Mr. Snow of Highgate, Dr. Yelloly, and Mr. McEnery. The first of these gentlemen was not only a frequent attendant at our meetings, but an assiduous collector of fossils and a donor to our museum, particularly after the excavation of the Highgate tunnel, during which operation he became possessed of a fine series of shells of the London clay.

Dr. Yelloly was a firm supporter of this Society at a period when it was struggling for existence under the auspices of our first Presi-

dent, Mr. Greenough, and real and efficient friends were put to the test. Dr. Yelloly was among the foremost of these as an active member of the Medico-Chirurgical Society, which body afforded the rising geologists their first place of meeting in Lincoln's Inn Fields, where our founders set up their standard of independence, and claimed to have an existence as uncontrolled by the Royal Society, as the medical men who aided them sought at the hands of the College of Physicians. The advantages which science has reaped from this independence of action and division of labour is now, indeed, admitted even by those who were opponents and have lived to see our success. In late years, as in early life, Dr. Yelloly was forward and at his post when any liberal measure was proposed connected with the progress of science; he took an active part in the formation of the British Association for the Advancement of Science, and when that body met at Birmingham he performed the duties of President of its medical section.

The Rev. J. M^{c}Enery, a Roman Catholic clergyman, and a zealous fossil osteologist, was first brought into geological notice by his labours in the bone-caves of Devonshire, near Torquay, where he resided. His prolonged researches in these caves produced an immense collection of fossil bones of the same species of quadrupeds as those which occurred in the celebrated Kirkdale cavern. The most striking inference from this collection, was a perfect demonstration of the agency of hyænas in collecting the herbivorous animals into caverns during long periods, proved by the absence of rolled bones and the abundance of fractured osseous fragments bearing the marks of having been gnawed by teeth; in short, confirming in a very remarkable manner the inhabited cave theory propounded by Dr. Buckland. Mr. M^{c}Enery's collection of the bones of British cavern quadrupeds, which is one of high merit, will, I understand, be soon disposed of to the public; and I trust that part of it at least will find a resting-place in our great national collection at the British Museum.

Palæozoic Geology.

silurian—devonian—carboniferous.

It was long after a true principle of classification, founded on the succession of organic life, had been applied to the tertiary and

secondary rocks, that the same method was used to work out the order of the oldest strata in which the remains of animals have been discovered. My own efforts, directed for several years to this end, have been so distinctly recognized by those whom I now address, as establishing a step by which the relative age of the older fossiliferous strata has been subsequently developed, that I ought to apologise for offering, on this occasion, even the shortest historical sketch of the process by which we have arrived at our present palæozoic classification. Some statement seems, however, to be called for, now that the subject is passing into many hands and into various countries. Having satisfied myself, after a labour of eight years, that I had amassed all the materials requisite to establish the existence of a sequence of rocks distinct from the Old Red Sandstone and Carboniferous Limestone, and having applied local names to each of the ancient formations so situated, I was strongly urged by many scientific friends, both at home and abroad, to propose some general name for the whole group. I fixed upon the ancient geographical term "Silurian," which was approved of, and has since been adopted, not merely in my own country, but in the most distant parts of Europe and in America*. No sooner, however, had it been proposed, than another seemed requisite to characterize the older slaty rocks, on which the newly-named Silurian system reposed. For these an eminent continental geologist suggested, in a letter to myself, the classical word "Hercynian," derived from the Hartz mountains, where the rocks might be presumed, from their antique aspect and mineral character, to be of remoter age than the soft argillaceous Silurian types of Britain. Alive, however, to the danger of mingling assumptions, drawn from lithological structure, with proofs derived from unequivocal succession of organic remains, and knowing that in our own country there was, in fact, a vast mass of slaty rocks on which the Silurian strata reposed, and which my friend Professor Sedgwick had long studied, I urged him in describing these rocks which he had made his own, to fix on a general British geographical name. He then

* When Ostorius, the Roman general, conquered Caractacus, he boasted that he had blotted out the very name of Silures from the face of the earth. A British geologist had, therefore, some pride in restoring to currency the word Silurian, as connected with great glory in the annals of his country.

adopted the name of Cambrian. Nothing precise was known at that time of the organic contents of this lower or Cambrian system, except that *some* of the fossils contained in its upper members at certain prominent localities were published, Lower Silurian species. Meanwhile, by adopting the word "Cambrian," my friend and myself were certain, that whatever might prove to be its zoological distinctions, this great system of slaty rocks, being evidently inferior to those zones which had been worked out as Silurian types, no ambiguity could hereafter arise. On the other hand, the adoption of any term derived from a part of the continent, where we had not made ourselves masters of the true sequence, might involve the whole subject in confusion. This would in reality have occurred had the word "Hercynian" been selected, for subsequent researches have taught us, that the greater portion of oldest rocks of the Hartz are younger than the Silurian system, and that their antique impress is due to metamorphic action*. In regard, however, to a descending *zoological* order, it still remained to be proved, whether there was any type of fossils in the mass of the Cambrian rocks different from that of the Lower Silurian series. If the appeal to nature should be answered in the negative, then it was clear, that the Lower Silurian type must be considered the true base of what I had named the *Protozoic* rocks†; but if characteristic new forms were discovered, then would the Cambrian rocks, whose place was so well established in the descending series, have also their own fauna, and the palæozoic base would necessarily be removed to a lower geological position.

In a very comprehensive memoir, recently read, which, when published, will throw a clear light over the ancient rocks of the lake districts, as compared with their equivalents in Ireland, Wales, and Scotland, Professor Sedgwick has answered this appeal himself‡. Re-examining all the ancient fossiliferous rocks in Cumberland, he has become convinced that they are there divisible into two great zones, referable to Upper and Lower Silurian types, the former surmounted

* See Geological Transactions, vol. vi. p. 288.

† Shortly afterwards Professor Sedgwick proposed the word "Palæozoic" as a general name for all the older groups, which, preferring to my own, I immediately adopted as involving no theory.

‡ Proceedings, No. 82, p. 541.

by old red sandstone and carboniferous limestone, and the latter reposing on some of the oldest sedimentary rocks of our islands, the Skiddaw slates, in which no organic remains have been detected. Numerous fossils from the Berwyn mountains, Snowdonia, and other Cambrian tracts, which he collected many years ago (but which, owing to the want of space at Cambridge, have been only lately unpacked), have been recently subjected to the same interrogatory, and have given the reply, that vast as the thickness of strata may be, the same forms of *Orthis* which typify the Lower Silurian rocks, not only range through what had been termed the Upper Cambrian (Bala, Berwyns, &c.), but also throughout the whole of North Wales.

In the mean time other observers had been working out detailed facts which pointed to the same conclusions. In a part of Cumberland, Mr. James Marshall had established the presence of Silurian deposits, where it was formerly supposed still older rocks prevailed, and more recently Mr. MacLauchlan of the Ordnance Survey, has shown us that all the slaty, and in parts metamorphic tracts of North Pembroke, which are coloured in my Silurian map as Cambrian, or in other words, as strata beneath the Llandeilo flags, contain many of the same forms as the Lower Silurian rocks. Before these inquiries had taken place at home, the researches of Professor Sedgwick and myself in Germany and Belgium, and of M. de Verneuil and myself in Russia, had led to the same conclusions, viz. that wherever it exists, the zone of fossiliferous strata characterized by the Lower Silurian *Orthidæ*, are the oldest beds in which organic life has been detected, and that many of the subjacent rocks, sometimes even when in the form of gneiss, mica schist, talc schist, chlorite slate, &c. are nothing but metamorphic rocks, in less altered parts of which the same typical fossils are observable.

If then our researches teach us that the term Cambrian must cease to be used in *zoological* classification, it being in that sense synonymous with "Lower Silurian," we see the true value of having established a type like the latter, which being linked on through intermediary groups to overlying formations, the age of which was previously well known, we have arrived *gradatim*, and without hypothesis, at the apparently true base of the zoological series in Europe. It is right, therefore, that I should announce that the conventional line which was set up in the map of the Silurian region, between the

Lower Silurian and the Cambrian rocks, and which has been adopted by Mr. Greenough, has no longer any reference to strata identified by distinguishing organic remains, for the same fossils are found in strata on each side of that demarcation. Such lines of division, however, when viewed as the signs of local phænomena, are notwithstanding highly useful, both as indicating changes of lithological character, great lines of disruption and lower divisions of the same palæozoic group. In short, all researches up to this day have led to the belief, that the Lower Silurian fossils were the earliest created forms, and that this "protozoic" type prevailed during that vast succession of time which was occupied in the accumulation of all the older slaty rocks, until the Upper Silurian period, when new creatures were called into existence, and when the earlier forms diminished and were succeeded by a profusion of chambered shells which so abundantly characterize that epoch.

This, Gentlemen, is I trust a good step gained. To establish upon sound data the true theory of organic succession in the oldest forms of life, is surely important, and we ought to rejoice that the British islands have afforded us the means systematically to work out the question. Ascending then from these lowest types, the Upper Silurian zone is one of great distinctness in England, and in the Baltic—in the northern provinces of Russia and in North America; the Wenlock, Dudley and Ludlow fossils having been abundantly found in both hemispheres. As soon, however, as we have advanced through this zone, a new era is announced by the presence of the earliest Vertebrata. The minute and curious fishes in the uppermost bed of the Ludlow rock, are the earliest precursors of many singular ichthyolites which succeed in that enormous formation, termed from its mineral character in Scotland and parts of England, the Old Red Sandstone. But in this as in nearly every other deposit, lithological characters are fugitive, and the red, green and yellow sands of the North, are found even in our islands, as in Devonshire and the adjacent tracts, to be replaced by black schists and limestones. But here again zoology enables us to interpret the language of nature, for it was merely by seeing the letters of the alphabet spread out before him in a cabinet, and without even having visited the country, that Mr. Lonsdale was led to conceive that a large portion of this tract, though very

dissimilar in mineral aspect, would prove to be of the same age as the Old Red Sandstone. I need not tell you how the researches of Professor Sedgwick and myself, which first indicated the presence of some members of the carboniferous system of that tract, afterwards confirmed these views, nor need I remind you that we have since extended them to various parts of Germany and Belgium, for the abstracts are already in your Proceedings and the memoir is about to appear in your Transactions.

I must here, however, acquaint you, that the paper by ourselves upon the Rhenish provinces is admirably illustrated by a description of the Devonian fossils of that region, prepared at our request by M. E. de Verneuil and M. d'Archiac, in which many new genera and species are established, and the group is delineated with closeness of research and profound knowledge of natural history. In the same communication these authors offer a general table of Palæozoic fossils, which in sustaining in the strongest manner the true intermediate character of the Devonian system, as suggested by Mr. Lonsdale, seems to be one of the most valuable documents yet presented to our consideration, in leading us to view the palæozoic rocks as a great tripartite series composed of the Carboniferous, Devonian, and Silurian systems.

Further, I would specially draw your attention to the enlarged views of our French coadjutors, derived from extensive study, in which they estimate the relative increase and decrease of various genera and species of fossils in the three divisions of the earlier periods, and show that whilst a few species (twenty only in upwards of 2750 distinct species or well-marked varieties) range throughout the tripartite series, yet that each system has a distinctly typical fauna, whether we derive our conclusions from researches in our own parts of Europe, or from an examination of American and Russian forms*.

Whilst speaking, however, of this table, I must at the same time do justice to one of our own countrymen, Mr. Austen, the value of whose researches in Devonshire you have had previous opportunities of estimating. I have recently seen a MS. table prepared

* M. de Verneuil has, with my full consent, enriched this general table of comparison by the addition of the names of all the new species and characteristic palæozoic types collected in our two visits to Russia, and the description of which we are now preparing.—March 1842.

approaching to our own. In that singular country, in which so large a portion of the existing terrestrial and marine fauna differ so essentially from those of every other region, it is curious to detect in the rocks many fossil Corallines and Mollusks* closely analogous to the Silurian species of the British Isles, thus adding another proof to many we already possess, that the same climatological and physical conditions were very widely spread during the earlier ages of the earth. Slender as our information is as yet respecting the natural history of that wide and detached continent which British industry is reclaiming, we cannot but anticipate a rapid accession to our knowledge, now that some highly-gifted naturalists are established in it. Whilst I simply allude to Mr. W. MacLeay† and Captain Philip King, whose researches are directed to branches of science connected with our own, it is my duty to mention more specially the Rev. W. B. Clarke, a member of this Society, who has previously contributed to our Proceedings and Transactions, and who in his recent voyage to Australia has afforded us fresh evidence that his leisure hours will still be employed in geological pursuits. A short residence at the Cape of Good Hope enabled him to communicate to us a memoir on the structure of that colony, which seems to confirm what we had previously learnt from Herschel and Smith concerning its northern limits, and leads us to conclude that rocks of the Silurian age constitute the chief sedimentary masses of the southern promontories, though often much altered by the intrusion of igneous rocks.

Having alluded to Australia, I cannot refrain from expressing my delight, that Captain Grey, whose sketches of his arduous journeys in the wildest portions of that land are already placed among our standard works of travels, and whose future researches are cer-

* See Mitchell's Expedition into the interior of eastern Australia, vol. i. chap. i.; vol. ii. chap. 15.

† Although Trilobites so characteristic of the protozoic æra have not yet been detected, my friend Mr. MacLeay acquaints me that he has recently recognized the first fossil crustacean found in Australia, a macrourous decapode, which being discovered by Mr. Emery of the Beagle, has been brought home by that officer, and through Dr. Fitton has found its way into our museum. "This crustacean (writes Mr. MacLeay) is nearly allied to Thalassina, and is interesting as a specimen from being the first fossil crustacean, and I believe the only one yet found in New Holland."

tain to enrich our knowledge, should happily have been selected to rear the nascent establishment of Adelaide, at the same time that a most valued member of our own body, Sir John Franklin, is rendering Van Diemen's Land a school of natural knowledge. Under the more euphonous name of *Tasmania* (derived from its real discoverer Tasman), the intrepid polar voyager, though now unaided by the great zoologist, the companion of his former toils, assembling together a few men of science and letters, has founded the "Tasmanian Philosophical Society," to the first Number of whose published labours, printed at Hobart Town, I beg to refer you as containing an introduction and several memoirs which would do credit to any Society in this metropolis. The geological articles contained in it refer only to the structure of Kerguelen's Land, and fossil wood from Macquarrie Plains; but as some very remarkable fossils of very ancient forms have already been procured from the vicinity of Hobart Town, I trust that the energy of the governor and his known devotion to our pursuits, will induce him to procure from some one of the intelligent scientific staff which surrounds him, a detailed account of the position and relations of these organic remains, the possession of a good suite of which is still a desideratum in the Geological Society of London*.

In estimating the progress of inquiry in this department of geology in our own country, the recent work of Professor Phillips upon the Palæozoic fossils of Devonshire and the adjacent tracts claims our special attention, not only on account of the talent which he has shown in describing many new forms, but also on account of the classification which he suggests. We are already signally indebted to this author for inquiries in various departments of

* The geological notices in the Tasmanian Journal of Natural Science, consist of a description of some silicified wood from Macquarrie Plains by Dr. J. D. Hooker, and a sketch of the mineral structure of the northern part of Kerguelen's Land by Dr. M°Cormick, both attached to Captain James Ross's expedition. The latter acquaints us that this tract is exclusively composed of trappæan (basaltic) and metamorphic? rocks, with the exception of certain truncated and dismembered beds of coal which are traceable at intervals, pretty much I presume, like the broken and isolated portions of coal which are found in the trap rocks of the northern end of the Isle of Skye.

geology, and especially for his volume upon the organic remains of the Carboniferous Limestone. Without the previous existence of that work, there might have been some difficulty in asserting that the Silurian is, as a whole, independent of the Carboniferous system. The recent inquiry is a part of his duty in a public office in which he is fortunately employed, and the suggestion of which does infinite honour to Mr. de la Beche, and credit to the government who sanctioned the appointment. From Devonshire the Ordnance geological forces, directed by these able leaders, have moved into the Silurian region. Doubtless, under such discerning eyes, and with such a number of assisting hands as are now turned into this formerly deserted tract, many new forms may be expected to appear. If, however, the Silurian catalogues should be much augmented and enriched by the labours now so vigorously directed to that point by government authority, I trust that geologists will pardon the omissions and defects of the person who first toiled to unravel the phænomena of that region, assisted only by a very few of its kind inhabitants*. Such personal considerations are, however, of little moment, and I pass from them to that which is of real importance, the establishment of the best palæozoic classification.

Now the first question is, have any such new lights been thrown upon the subject of the older rocks by the recent work of Mr. Phillips upon Devonshire, as to change the nomenclature previously adopted both at home and abroad, and to substitute for it that proposed by Mr. Phillips, namely, Upper, Middle, and Lower Palæozoic strata? I confess that as I read this volume I perceived none, ex-

* In preparing my work I derived much assistance from a valuable original MS. on the Structure of Shropshire by Mr. A. Aikin, the earliest modern geologist, who, with his associate Mr. T. Webster, worked in this field; whilst my chief co-operating friends were the Rev. T. T. Lewis of Aymestry, Dr. Lloyd of Ludlow, and Mr. Davies of Llandoverey. It is, however, to Mr. Lewis that I am more indebted than to any other person, for he had acquired a very accurate knowledge of the order of the strata of his neighbourhood before I visited it. He was, indeed, my companion in the field in visiting several important localities, and as I can truly say "hæc meminisse juvat," I sincerely thank a friendly critic in the Edinburgh Review, April 1841, for having dwelt upon these facts in the history of the "Silurian System."

cept that after describing the species, the author shows that the fossiliferous strata of the Eifel are the equivalents of those of South Devon, a point, however, which had been previously established by Professor Sedgwick and myself.

Adopting from ourselves the word "Palæozoic," Mr. Phillips extends however its original meaning, and applies it to all the strata containing organic remains, from the oldest formation to the Magnesian limestone inclusive. His Lower Palæozoic rocks are admitted to be exactly synchronous with those which were worked out as types under the name of Silurian, and yet he entirely omits that term in his parallel table of equivalents, in which he styles them "Transition and Primary Strata;" whilst for the *ordinary* names to parallel with his "Middle Palæozoics," the much newer terms of Eifel and South Devon are made use of—terms of comparison, it will be recollected, which were introduced by Professor Sedgwick and myself *long after* the establishment of the Silurian type. I ask those geologists who supported me by their approbation throughout my labours, if the name first proposed by him who worked out and defined a system of classification, is to be suppressed when not only no evidence is brought to disprove its value, but when succeeding observers in various parts of Europe and America have sanctioned it. But as this is now simply a subject of nomenclature, and my facts are not disputed, let us see whether for all the practical purposes of our science, the term Silurian, as first proposed, ought to be preferred, in use, to the term "Lower Palæozoic," which is to supplant it. The word was chosen because it was liable to no misconceptions, and never could lead to false theoretical deductions. It is, as before stated, simply a geographical name, derived from a region containing newly defined types of succession. When subsequently we used "Palæozoic" as a comprehensive term for all the older rocks, Professor Sedgwick and myself intended to apply it generally to that great series which embraces the Carboniferous, Devonian or Old Red Silurian and Cambrian groups.

In extending the palæozoic range so as to include the magnesian limestone, Mr. Phillips does so because that formation contains some species of Producti very analogous to carboniferous forms. But he knows well that rocks of the same age in Germany and in our own country, contain the remains of several species of Saurians, and [illegible]

recent exploration of Russia (1841) further establishes the important fact, that deposits in the very same place in the series as the magnesian limestone, and loaded with Producti, are *also* charged with Saurians. What, then, are the *zoological* bases which ought to define the boundary lines between large groups of strata? Are they the vertebrata or invertebrata? If such a great feature of change in animal life as the earliest appearance of Saurians is to be taken as the limit of one vast geological division, we must exclude the magnesian limestone from the older series, and Mr. Phillips's proposed extension of the term Palæozoic cannot be sustained. Adopting this principle of the vertebrata as our guide, we may go on to say, that the true Silurian type ceases in the ascending order at that band of rocks which, in truth, forms the very uppermost layer or summit of the Silurian strata, in which the lowest order of vertebrata or fishes first appear, and then having ascended through another vast series, loaded with peculiar ichthyolites, we may announce a new æra in the magnesian limestone or zechstein, where we meet in the Saurians with another and higher class of the animal kingdom, wholly unknown in the inferior beds. It was, I beg to say, on this principle that I formerly proposed to divide the strata of England into seven great systems, as expressed in the small map of England which accompanies the map of the Silurian region. I do not assert that this general classification of the British geological series should be preferred to that of Mr. Phillips. He may contend that the universally distributed mollusk affords a more useful horizon line than any class of the higher order of animals. I merely state the case, and I hope fairly, to show that whether geographical terms be ultimately adhered to or rejected, all nomenclature founded *solely* upon our present knowledge of the distribution of animal and vegetable life, must be liable to change with every new important discovery, whilst that terminology which involves no such hypothesis, but is simply based on the proofs, that within a given region certain groups of beings are included, can never be gainsaid. It is on these grounds, therefore, that I am encouraged to hope, that the word "Silurian," which has been warmly sanctioned by the classic authority of Von Buch, which E. de Beaumont and Dufrénoy have engraved upon their splendid map of France, and which our fellow-labourers in America have adopted, will not be obliterated to make way for

other names which are not founded upon any *new* distinctions, stratigraphical or zoological. So long, gentlemen, as British geologists are appealed to as the men whose works in the field have established a classification, founded on the sequence of the strata and the imbedded contents, so long may we be sure that their insular names, humble though they may be, will, like those of our distinguished leader William Smith, be honoured with a preference by foreign geologists, who, looking from a neutral ground, are sure to be the most impartial judges. The perpetuity of a name affixed to any group of rocks through his original research, is the highest distinction to which any working geologist can aspire. It is in truth his monument, and therefore, gentlemen, I trust you will pardon me if I have occupied you too long with the allusions to this point, and which have been elicited by the work of one for whom I entertain so high an esteem as Mr. Phillips. I will therefore only add my hope, that now when the term Silurian has been so widely spread, the Director of the British Geological Ordnance Survey, who encouraged the author of the 'System' to propose a separate name for the types he had worked out, will not permit the labours of his friend to be submerged, and thus seem to convey to foreign geologists the idea, which is indeed far removed from the truth, that there are any real differences between his views and my own on this important subject. In terminating these considerations, I beg geologists to recollect, that I never entertained the idea that the local types around Ludlow and Wenlock would be found applicable in detail to strata of the same age in distant places; on the contrary, having shown that even within a very limited radius such subdivisions varied with varying conditions, it was a leading and constant object of my work, to demonstrate that the broad divisions of Upper and Lower Silurian alone, could be maintained as terms of distant and foreign comparison.

There are two short communications by Mr. Lyell on the older rocks to be noticed. The first is on the strata between Aymestry and Wenlock, in which he dwells on the assistance to be derived respecting the amount of dislocation in strata, by attentively noticing the deviation from a vertical position of the inclosed corals; but he adds that great caution should be used to distinguish between those specimens which may have been torn from their position with

reference to the horizon while growing and inverted, and those which have lost their original mode of growth by subsequent dislocation of the strata. From the known habits of recent corals, Mr. Lyell also infers that the Silurian strata must have undergone successive depressions during their accumulation, as beds of Polyparia belonging to the Wenlock limestone are overlaid by many hundred feet of sedimentary matter. In the second communication Mr. Lyell offers some remarks on a series of fossils from the neighbourhood of Christiania, and he infers from the evidence they afford, that the limestone to which they belong is of the age of the Lower Silurian rocks; and on similar grounds he places the limestone of the island of Langoen, one of the highest beds of the country, in an intermediate position between the upper and lower Silurian rocks, constituting a passage from the one to the other.

The last memoir which has been read before us on the British Palæozoic rocks, relates to their development in a part of Westmoreland, and is from the pen of Mr. D. Sharpe; and I rejoice to see so clear and systematic a workman enlisted in the survey of the older rocks. Agreeing on some essential points with Professor Sedgwick and Mr. James Marshall, particularly in reference to the superior and inferior limits of the Upper Silurian group, this author, who had previously made himself acquainted with the best types of the Silurian rocks, conveys to us additional details of this interesting tract, in which he has distinguished upon a map the Upper Ludlow rocks, as characterized by many fossils, from an inferior slaty formation which lies between them and calcareous bands charged with Lower Silurian fossils. Dividing this intermediate formation into three sub-groups (by only mineral characters however), he gives to the whole the local name of "Windermere Rocks,"—a term which I understand he only uses until by the discovery of fossil evidences he may be able to refer these beds to their proper Silurian equivalent. If I were allowed to judge from the experience of one visit to a part of the country described by Mr. Sharpe, in which I found Orthoceratites in mountains marked by him as "Windermere Rocks," and also from his own showing, that these rocks are included between types of the higher portions of the Upper and Lower Silurian strata, such intermediate formation must be on the parallel of the Wenlock strata, which in many parts of the Silurian

region, as well as in the North of England—(*i. e.* wherever the subdividing limestone and fossils are suppressed) can only be recognized under the general term of lower members of the "Upper Silurian Rocks." As Mr. Sharpe proposes to revisit the country, and to extend his researches from Westmoreland into Lancashire and Furness, he will have ample opportunity of confirming or rejecting my surmise. In regard to that portion of the memoir which points out the existence of many faults and anticlinal lines, I am not prepared to say to what extent they accord with the previous observations of the great geologist of the lake country, Professor Sedgwick, or of his precursor, Jonathan Otley.

I would now speak of a work which has recently appeared, entitled 'The Old Red Sandstone, or New Walks in an Old Field.' From a pretty accurate acquaintance with the tracts from which Mr. Miller has taken his title, I can assure you that the walks of this author had been little trodden, and that his claims to originality are very just. It is impossible to peruse his pages without delight in tracing how the strong mind of Mr. Miller has enabled him to rise step by step from the stone quarry of his, and I may add my own, native county Ross-shire, to a place in literature and science which few reach, even with all the support derived from an expensive education; or without admiring the ability with which this unassisted observer first succeeded in putting together the dislocated fragments of the very singular fish, called Pterichthys by Agassiz, long before that creature was first understood. Look again to the clear and general view which this author takes of the greatest of Scottish deposits, and how well he conveys to unpractised readers a true idea of its position, importance, and divisions, and you will agree with me that in Mr. Hugh Miller we have to hail the accession to geological writers of a man highly qualified to advance the science. Few persons, and too often least of all those who are, if I may so speak, professed geologists, succeed in imparting to others, who have not studied the science, a clear conception of their views. In this respect the character of Mr. Miller's work is admirable, for it portrays the means by which the author acquired his knowledge, and, from its persuasive manner, is worth, to a beginner, a thousand didactic treatises.

I hoped before now to have seen in print the very valuable me-

moir prepared by Dr. Malcolmson long ago, on the divisions and development of the Old Red Sandstone in the North of Scotland. This delay has been caused solely by the desire that the description of the various fishes which he has pointed out as characterizing the different stages of the deposit should be given by Professor Agassiz. The numerous avocations in glacial and other geological inquiries, as well as, I regret to say, his partial ill health, might alone have led us to account for the postponement of this labour by M. Agassiz; but in a recent letter to myself he has also given the following important reason:—"When I promised you to occupy myself with the determination of the fossil fishes of Dr. Malcolmson, I believed that it would be as easy a task to me as the determination of other ichthyolites, and I had no doubt that your Devonian system must reveal quite a new world in the class of fishes so very different from existing species. The effort has thrown upon me the obligation of prodigious labour, to arrive at some precise results as to these curious objects; and without giving you something very imperfect, which I look upon as yet to be unworthy of publication, I must have recourse to your indulgence for the delay."

Regretting sincerely that injustice seems to be done to Dr. Malcolmson by this delay, I have, I confess, a pleasure in knowing that Professor Agassiz will well investigate all these curious animals before he pronounces his final opinion. I can even assure him, that strangely formed as these Scottish types may be, he has yet to hear of some still more marvellous fishes which the Devonian or Old Red system contains in assuming its Russian dress. In that empire, where in some mountain tracts the system is black, slaty, and crystalline, there are also vast undulations and plains in which it is composed of slightly coherent, red, green, and yellow sands, shales, and limestones. In some of these beds, near Dörpat, Professor Asmus has detected gigantic fishes, which he is now describing; and Mr. Pander, so distinguished by his palæontological works on the environs of St. Petersburgh, is preparing an account of others, some of which are specifically identical with those of Scotland. I cannot venture to anticipate what these naturalists will shortly lay before the public, but I may be excused from announcing, that the moment I exhibited to Professor Asmus some drawings of the Scottish old red sandstone fishes, his eye at once fell upon the Pterichthys

e evidence afforded by the comparative dimensions of one
f Labyrinthodon found in the same quarry, Professor Owen
shows that the anterior and posterior extremities must have
disproportionate magnitude, according well with those of
rotherium, and he therefore infers, and with great apparent
that the Labyrinthodon and the Cheirotherium were one
In a second memoir upon certain remains from the Oolitic
e has established a genus of Saurians equal in size to the
nd in a third upon the remains of a crocodilian Saurian
"Lower Greensand;" he concludes, from the same uner-
dence in the form and texture of the bones and teeth,
are quite distinct from any Saurian hitherto described;
herefore refers them to his new marine genus Polypty-
Whilst I delight in seeing that the tenants of those ancient
ave met with so competent an expositor, I cannot but re-
my place should not at this moment be occupied by our
ybeare; for the founder of the genus Plesiosaurus would
ght you to admire a multitude of comparisons and osteo-
djustments contained in the results of Mr. Owen's researches.
unequal to enter into a discussion of his merits, I can, how-
common with all my brother geologists, express to him my
se of gratitude for the successful efforts he has made to
to us new links in the scale of nature's works.

e subject of Saurian remains our knowledge has also been
during the past year by Dr. Mantell, in a memoir commu-
o the Royal Society, on the lower jaw of the Iguanodon,
he remains of the Hylæosaurus and other Saurians disco-
the strata of Tilgate Forest. Not pretending to have ade-
quaintance with the subject treated of by Dr. Mantell, I am
our old and valued associate is once more before the pub-
ne of those original researches with which, during the last
ve years, he has so much enriched our science, and which
ained for his name so high a place in the volumes of the
vier, as to render any eulogium on my part superfluous.
he arduous labours of a man, who, like Dr. Mantell,
few leisure hours at his disposal, first in discover-
ing from their stony bed, and lastly in describing
n bound to observe that such merits deserve, as

they have obtained, the highest praise which working geologists, like ourselves, can offer. In thus estimating, however, the value of Dr. Mantell's researches, I must be permitted to say (and in the most friendly spirit), that whilst I understand the propriety of the motive which led him to communicate his last memoir on the Iguanodon to the same Society to which he had addressed his first account of that Saurian, I regret that he should not have communicated to ourselves other palæontological memoirs, the consideration of which, I must say, pertains particularly to the Society over which I preside. So long as the Royal Society produces volumes adorned by the writings of the first mathematicians, physiologists, and chemists of the age, so long will it maintain its high place, little heeding our humbler pursuits.

Two memoirs have been read before us to illustrate the celebrated "bone-bed," which, lying at the base of the lias, and in contact with the uppermost members of the new red system, has hitherto been classed with the former deposit. The first of these, communicated by Sir Philip Egerton, is entitled "On the occurrence of Triassic Fishes in British Strata;" the second is "On the occurrence of the Bristol Bone-bed in the Lower Lias, near Tewksbury," by Mr. H. Strickland. The fact to which Sir Philip Egerton adverts is, that out of a series of specimens from this bed at Axmouth and Aust, M. Agassiz determined four species to be well-known forms of the Muschelkalk, whilst fifteen were unknown in that deposit or any other part of the triassic group; and Sir Philip concludes, that the beds in question ought to be removed from the lias, not only because the fishes are specifically distinct from those of that formation, but because the forms of the ganoidians possess the heterocerque tail, a form which the classification of Agassiz confines to deposits of higher antiquity. This reason ought to have great weight, and might, if unconnected with others, at once dispose us to move our base line of the lias some few feet higher.

A fresh-cut section of the Gloucester railway had exposed at Combe Hill, near Cheltenham, the same singular bone-bed which is so well known at Axmouth and at Aust. From an intimate knowledge of that country, I can recognize the fidelity with which Mr. Strickland identifies certain thin layers of sandstone and grit at the bottom of the lias extending to the north with the adjacent bone-bed, which in its

further extension loses those ichthyolite characters for which it is so remarkable over an area in our isles as wide, indeed, as that of the famous "Kupfer Schiefer" in Germany. Now in Gloucestershire the bone-bed described by Mr. Strickland contains not only fishes, many of which are of new species, but also many shells, some of which are supposed to be of forms intermediate between those known in the lias and the keuper. In this case, therefore, we are probably in the same position as the inquirer into the Palæozoic rocks, who stands upon the beds of passage from the Silurian into the Old Red or Devonian rocks before adverted to. In both cases, when he finds forms which belong to the inferior and superior systems, whether he may draw his boundary line above or below these equivocal strata, seems at first to be of small importance: for, as with the progress of research, we must expect to find an infinite number of strata which contain fossils indicating a transition from lower to higher formations, so must the lines of separation which geologists set up between formations be liable to undergo small alterations. Adhering, however, to the belief, that in the sequel those limits will most prevail which are most made to depend on great changes in animal economy, I think that the conclusion of Sir Philip Egerton, as based on the existence of the fishes with heterocercque tails, must lead us to place the "bone-bed" as the uppermost limit of our New Red System, or in other words, as the last-formed stratum in which such ichthyolites appear.

A point connected with an important previous deduction has been determined by Mr. Strickland in a cutting of the Gloucester railroad. The period at which the Lickey trap rocks were erupted, is now proved by actual sections to be that which from collateral circumstances had been surmised by myself. By observing that the New Red Sandstone of the Upper Lickey lies unconformably upon a mass of Red Sandstone, Mr. Strickland has demonstrated that the disturbance and elevation of the ridge took place after the deposit of the Lower New Red Sandstone, and anterior to the accumulation of the New Red, properly so called. In this fact some geologists may see an additional reason for classing the Lower New Red Sandstone with the coal-measures, both having partaken of the same elevatory movements. Though such a consideration alone might not to guide us in classification, the facts so recently put forward by Professor Sedgwick of the prevalence of

carboniferous species in this red sandstone, both in Cumberland and in Warwickshire*, and the similar data, which I ascertained in Staffordshire, Shropshire, &c., may eventually lead us to consider all the sandstones beneath the magnesian limestone as naturally connected with the carboniferous æra, a view which my last researches in Russia have also led me to adopt. In this respect, indeed, the deposit agrees well with the rothe-todte-liegende of foreign authors, which, like our Lower Red Sandstone, contains both carboniferous plants, and occasional thin seams of coal.

From Mr. Trimmer we have received an account of the true geological position of the *Cucullæa decussata*, verifying that which was originally assigned to it by Mr. Webster, and confirming the justness of Mr. Parkinson's opinion, that the species is distinct from the Cucullææ of the greensand, though in some more recent publications the Faversham fossil has been considered identical with the Cucullææ of Blackdown.

TERTIARY ROCKS.

An important addition to our knowledge of the relations of the Tertiary rocks of Europe proceeds from the pen of Mr. Lyell. On comparing the fossils of the *Faluns* of the Loire with those of the Cotentin, and again, by a comparison of both with the crag of Suffolk, Mr. Lyell has corrected a view which he had formerly adopted, that these deposits were not formed during the same epoch. By an attentive examination of different tertiary localities in Normandy, some of which seem to have escaped the notice of former observers, he has ascertained the existence of many of the true Suffolk crag fossils in deposits extending southwards as far as Sainteny. He then describes the *Faluns*, properly so called, at Dinan, Rennes, Nantes, Angers, Doué, Sevigné, and the tracts S. and S.E. of Tours, in some of which the great abundance of corals and echinoderms, and the small number of mollusks, present a perfect analogy to the white or coralline crag of Suffolk, though the fauna is quite distinct in species from the fauna of the coralline crag. From the existence of a number of detached points of *Faluns*, Mr. Lyell infers that a large part of France, now drained by the Loire and its tributaries, was submerged during the Miocene period. Finally, he convinces

* See Geological Proceedings, November 1841.

himself that all the shells of these French deposits belong to one group, and that they are really contemporaneous with the crag of Suffolk, though there may be shades of difference in their relative ages. It is well to observe that so sound a geologist as Mr. Lyell does not shrink from identifying two distant deposits in which eighty-five per cent. of the fossils are of distinct species, fifteen species only being found common to the two, because he shows that both these deposits correspond exactly in the analogy which they bear to the fauna of the present day. Having also detected freshwater and land remains in the intervening tract, Mr. Lyell further offers us a satisfactory explanation of how the Miocene *Faluns* of the Loire and our Suffolk crag should be contemporaneous deposits and yet so different in contents, the seas in which they were respectively accumulated having been separated by dry land; that in which the crag was deposited opening to the north, and those in which the *Faluns* were accumulated opening to the south.

Mr. Lyell's works being before us, I seize this opportunity of congratulating the Society that a geologist possessing his powers of classification should now be occupied in studying the structure of North America. In that wide field, in which for the last few years the native observers have been gathering together both a vast profusion of valuable detailed sections, as well as many general comparisons with our own divisions, it is impossible that a good European geologist can fail to reap an abundant harvest; and whether it be in his own tertiary domain, of which he has so largely extended our knowledge, or by grappling with the Palæozoic rocks, which in that vast continent are developed on so splendid a scale, our science is sure to profit from such a revision as our associate will be enabled to present to us. He has indeed already given us an earnest of his future communications, first in a letter to Dr. Fitton, on the older deposits in the state of Pennsylvania, and cites evidences in one tract confirmatory of the theory of terrestrial and lagoon origin of coal-beds, which was pointed out by Mr. Logan, who, having led the way in this inquiry, is now extending it in America. Notwithstanding the real value justly attached to these views, which have been supported by the labours of Mr. De la Beche, and which received an ample illustration in the last discourse of Dr. Buckland, I must caution geologists against applying this

theory generally to all coal-fields because it has been found true in some, for it is manifest, that in those tracts (and they are numerous and large) where marine shells, ironstone and shale, filled with large fishes, alternate with beds full of plants, confusedly piled together, it will be impossible to account for the origin of coal by subsidence or overflow of masses of vegetation *in situ*.

In a recent communication on the Falls of Niagara, Mr. Lyell has taken the opportunity of explaining the sections of the American geologists who have described them, from Mr. Amos Eaton, who first showed the order of the strata, though his comparisons with British types were erroneous, to those of Conrad and James Hall, who have successfully placed these groups in parallel with our own Silurian strata. In showing the varied alternations of the hard and soft rocks which form the Silurian system of that region, and the exact inclination of the strata, Mr. Lyell exhibits chronometers of the probable retrocession of the falls, indicating where the river has worked back more rapidly when it had to recede through soft shale and sand, and how the solid barriers of limestone have presented greater obstacles. These data are indeed only more correct and more detailed illustrations of the general phænomena advocated by Bakewell, De la Beche, and the American geologists, that the recession is chiefly due to the water undermining soft shale and sand from beneath ridges of harder rock which are successively plunged into the abyss. It is well however to observe, that, from an inspection of the country, Mr. Lyell has modified his former view, that the letting off or bursting of the Lake Erie might be the ultimate result of the retrocession of the Falls, for he now seems to incline to the belief, that owing to the nature of the strata through which they will have to work back, the final result will be the formation of long and dangerous rapids; while he justly points out how the formation of canals and the demand of water for the use of the lower country, which is passing from a state of forest to one of cultivation, will cause a gradual diminution of the upper lakes, and thus prevent a future catastrophe. But the chief point of interest in this memoir seems to me to be the inference deduced from the occurrence of beds of ancient fluviato-lacustrine shells near the top of the cliffs bounding the defile of the Niagara, and necessarily high above its present bed, that the river has worn down its channel through a tract,

in which the former water-courses (probably a succession of lakes or lake rivers) flowed on a much higher level; and he gives a strong reason for believing that the river has been the chief agent in this denudation, by stating that the channel in which it flows is not in any part the scene of dislocations or faults.

MICROSCOPICAL RESEARCHES.

The microscopic examination of fossil bodies was much enhanced in value when D'Orbigny astonished us by its application to the smaller cephalopods or foraminifera of the tertiary and cretaceous rocks, and by presenting us both with valuable descriptions and enlarged drawings and models. The discoveries, however, of Ehrenberg, and the much higher magnifying powers employed by him, opened out as it were a new former world of life, when he proved that certain strata were almost if not entirely composed of *Infusoriæ* so minute, that millions were included in a cubic inch of rock. In advancing his observations, this naturalist has recently asserted that certain species of animals of this class, which are *now living in seas and estuaries, were in existence when the cretaceous rocks were formed.* This announcement cannot but fail to arouse the lively attention as well as the surprise of geologists, who, relying upon what all the other departments of palæontology had developed, had come to the belief, that no form now living was created until after the completion of what are termed the Secondary rocks. If this discovery of the illustrious Prussian be substantiated, we see in it another proof, in addition to those which I have adduced in the previous pages, of the danger of as yet attempting to establish a nomenclature founded solely on the *fauna* and *flora* of former conditions of the planet. No terminology appeared less likely to be shaken than that proposed for the tertiary rocks by Mr. Lyell, nor could more time, thought and caution have been bestowed than he gave to the consideration of the names for the subdivision of the Tertiary Series, as founded on a great philosophical view. Whatever objections some persons might entertain to the upper divisions of his system, the characters of which were made to depend on a greater or less per-centage of existing species, there could be little doubt, from the multitude of previous researches, that his term "Eocene" was at all events secure from criticism. Many practical geologists believed that the close of the

shaded off so as imperceptibly to connect the Secondary and Tertiary states of organic life.

In our own country this department of the science, which is in a state of great advancement through the labours of Owen, Brown, Stokes, and other naturalists, has been cultivated with much zeal in one department by Mr. Bowerbank. Having formerly shown that the flints and cherts of the cretaceous system were originally composed (at least in great part) of *sponges*, he has lately pointed out, that the fossil bodies in question did not differ as he had supposed from the horny sponges of commerce, having recently discovered siliceous spicula in the latter. After a detailed and laborious examination of moss-agates and jaspers from Oberstein, Sicily, and Hindostan, he sees in them all the proofs, more or less distinct, of tubular fibres—of what he believes to be gemmules—and the existence of vascular structure, and hence he infers, that sponges have had a still greater share than he originally supposed in the production of the solid strata. In the Egyptian jaspers Mr. Bowerbank detects between the layers composing a specimen hundreds of *foraminifera*, often difficult to distinguish from species known in the calcaire grossier of Paris. Though as geologists and mineralogists we may be startled by the announcement of signs of former life in the geodes of Oberstein, because they are certainly, like our trap nodules of Scotland, inclosed in rocks of plutonic origin, I am quite prepared to admit, with Mr. Bowerbank, that in many jaspers, at all events, the microscope should develope former types of life.

When we consider the short period which has elapsed since these, the very minutest secrets of our solid strata, have been revealed to us, and by how few inquirers they have been studied, we may well admire the results. At the same time, seeing the great difficulties attending the study of these minute bodies, and the possibility that a certain amount of error *may* arise from the examination of such of these organisms as are imperfect under very high magnifying powers, I quite coincide with your late President, that we ought not to adopt too rapidly all the conclusions of the microscopists, however we must cordially thank them for the steps they are endeavouring to establish.

PROVINCIAL GEOLOGICAL SOCIETIES.

When presiding over this Society ten years ago, I congratulated my associates on the increasing taste for our science by the rapid rise of provincial scientific institutions*. I will not now endeavour to enumerate all these Societies, since through my ignorance I may omit to mention some which are well entitled to notice; but I will simply advert to two of the most recently established of these bodies, and whose objects are exclusively the same as our own, viz. the Manchester Geological Society, and the Dudley and Midland Geological Society.

The first of these, presided over by Lord Francis Egerton, has just published the first volume of its Transactions, which contains much good local geology, from the pens of our deceased member Mr. Bowman and Mr. E. W. Binney, and valuable descriptions of fossils by Capt. T. Brown. I am glad to find that the shells delineated for the first time in this volume, and which occur in the lower red marls at Collyhurst near Manchester, are now admitted to be in beds, which are equivalents of the magnesian limestone, an opinion it will be recollected which was expressed when these fossils were first brought to our own halls by Professor Sedgwick and Mr. Phillips†, thus offering a fresh proof that with newly-discovered lithological conditions, the same formation is often found to be diversified with remains unknown to us in the rocks of the same age which preserve their ordinary mineral characters.

Of the still younger Geological Society of Dudley, I have sincere pleasure in saying, that its first anniversary festival, at which I was requested to deliver an inaugural address, was eminently successful in uniting together the gentlemen of property in the neighbourhood with practical miners and fossil collectors, and there can be no doubt that an establishment so supported, and which is founded on ground so replete with countless subterranean phænomena, must have an honourable and a useful career. I refer you to the excellent Report of the Dudley Provisional Committee, a perusal of which, whilst it acquaints you that their museum contains some unique specimens and many worthy of a visit, will convince you that it is

* Geol. Proceedings, vol. i. p. 377.

† See Geol. Proceedings, vol. ii. p. 392, and Silurian System, p. 50.

directed by men of scientific discernment and zeal, who can well describe and appreciate the value of such a collection.

I rejoice in the formation of these provincial societies, being convinced that they will work out details of great ultimate value; and whatever may be the objections to free trade among nations, I have no hesitation in proclaiming the benefits of free trade in geology, because I know that our own volumes have risen in value, and our ranks have swelled in numbers, with the birth and growth of our younger friends and rivals.

FOREIGN GEOLOGISTS—PRUSSIAN SCHOOL.

Let us now consider the progress which our science has been recently making on the continent of Europe.

The visit of the King of Prussia to our country upon the auspicious occasion arising out of the birth of our future Sovereign, was marked by an event most gratifying to our feelings. To testify to His Majesty your sense of his gracious and warm patronage of the cultivators of geology, and "to prove that English geologists can never forget the deep obligations they owe to the land which has produced a Humboldt, a Von Buch, and an Ehrenberg*," you elected His Majesty a Fellow of the Society. The condescension with which His Majesty subscribed our obligation book, and the interest with which He examined our collections within these walls, will be remembered by us with just pride. Attended by the great philosophical traveller whose researches have opened out the widest fields to the inquirers in every department of Natural History, we who have drunk at the fountains of knowledge poured forth by Humboldt, must indeed rejoice in the day when our veteran associate appeared in our halls as the chosen friend of the Prussian monarch. Honour be to the King who has the wisdom and discernment to attach such a man to his person and his fortunes! Any effort of mine to do justice on this occasion to the eminent services which Baron A. von Humboldt has rendered to science, would be both presumptuous and misplaced; but I must seize this opportunity to assure you, that if his valuable life should be prolonged for a short term, the public will be furnished with convincing proofs that his brilliant mind can yet

* The above words were spoken by the President in admitting His Majesty as a Fellow of the Geological Society.

confer on us the choicest gifts. Let others more competent to the task dwell on the high merits of his inquiries into the distribution of terrestrial magnetism and various branches of physical science which have already appeared, or are nearly ready for the press, in a stupendous work embracing nearly all natural knowledge; be it for us, however, to estimate the skill with which he has developed, and the power with which he has applied the laws of climatology and physical geography to explain many problems in the earth's structure.

Having myself been favoured with the perusal of some pages of a work on the distant parts of the Russian empire, which will very shortly be published, I venture (however incompetent, to offer an adequate analysis of its merits) to assure you, that this work will shed fresh lustre on the head of its author and of his associates Rose and Ehrenberg, in elucidating the metamorphism of rocks, the origin of gold veins, and the epoch of formation of the gold alluvia of Siberia; whilst in expounding the great sources whence the civilized nations of antiquity derived their precious metals, Humboldt, the geographer, the geologist, the botanist, the man of universal science, will appear before you as an antiquary and etymologist, not inferior in erudition even to his late illustrious brother.

In correcting the errors which had crept into our maps respecting the direction of the great mountain-chains of Central Asia, he places before us, and on the grandest scale, a striking coincidence between the state of mineralization of various parallel *meridians*, or N. and S. chains, and happily contrasts them with the different characters of those which have an east and west direction.

These splendid generalizations, like others previously known to us, results arising from a long life of scientific research, are of so extended and diversified a character, that whilst we all applaud, few of us are capable of justly estimating their whole bearing upon the progress of science.

Although his duties to his sovereign alone prevented our conferring upon this great chieftain in science an honour commensurate with his high deserts*, as Englishmen we may always reflect with delight, that when Humboldt appeared among us he received the

* Allusion is here made to the proposed national British scientific festival in his honour, which Baron Humboldt was compelled to decline.

universal homage which is so justly his due, and which his enlightened and benevolent monarch must have been proud to acknowledge as one of the highest compliments we could offer to himself and to his people.

In explaining the motives which induced the Council to award the medal of this year to M. von Buch, I have necessarily dwelt not only upon the former great services rendered by that eminent geologist, but also to his recent palæontological works. So actively indeed is he employed, that even whilst I write he is preparing a monograph on the genus *Productus*, thus offering fresh evidence of his sagacity and indefatigable research.

Ehrenberg, to whom I have elsewhere alluded, is daily adding to his conquests over the invisible realms of nature, and Gustaf Rose has written on the metamorphism and mineral structure of the Ural with so much ability, that it will be my special business to dwell at some length on this topic on another occasion.

In the construction of improved geological maps of various parts of the Prussian dominions, Professor von Dechen still pursues his useful and meritorious career. His large and detailed map of the Rhenish provinces, in which he has been aided by Erbreich and other good geologists of the Prussian school of mines, is I believe completed. Two years ago M. von Dechen kindly furnished me with an unfinished copy, which has served as the model, from which has been taken the small map prepared for the Transactions to illustrate the memoir on the Rhenish provinces by Professor Sedgwick and myself. You must not, however, Gentlemen, judge of the very high merits of the original from the reduced skeleton map which we publish, and I beg you to consult the former as one of the most valuable documents of this nature yet offered to the public, particularly in the elaborate delineation of every variety of igneous, metalliferous, and metamorphic rocks, in a region so strikingly replete with them. Silesia has also occupied much of the time of M. von Dechen, in some districts of which he has marked the existence of bands of carboniferous limestone as distinguished from the Devonian, Silurian, and older members of the palæozoic series.

Oeynhausen, the old associate of Von Dechen, and so well remembered by many of us, has recently bored in search of salt springs

through upwards of 1000 feet of lias near Pyrmont, a fact which ought to teach us great caution in estimating what may be the maximum thickness of deposits. In our own country, the accurate method which Mr. De la Beche employs to test the thickness of deposits, will eventually give us, I trust, close approximations to the facts; and I learn from him that some of the ancient strata (the carboniferous for example) which have been accumulated in basins are enormously more thick than we had supposed, whilst others extending, like the Old Red Sandstone, over wide areas in lofty escarpments, will not prove to have those dimensions we had assigned to them. When, indeed, we consider that all shales, sandstones, &c. were once nothing more than the blue and black, and red mud, or sand which occupied the bottom of seas in former epochs, it seems as difficult to decide from general observations on the maximum thickness of any great deposit, as it would be to insist on the utmost depth of the ocean without the survey of the hydrographer. The borer and the field engineer must therefore combine to enable us to speak with precision on the vertical dimensions of strata.

RUSSIAN AND NORTHERN SCHOOL.

Not having yet personally visited Sweden, Norway, and Denmark, I am not prepared to say what progress our science has recently made in these states, but I may remark, that the beautiful map of Norway by Keilhau has scarcely received the attention which it merits; and we may be sure that the countries of so good geologists as himself and our associate Forchhammer, cannot be lagging behind in the general onward movement.

In regard, however, to Russia, I am enabled to speak with some confidence, after the two visits which I have paid to that country. Gratified as we were, not only by the most hospitable reception, but also the kind assistance afforded us by every Russian, from the Emperor to his humblest subject, it was a real source of delight to my associates and myself in our first visit to trace throughout the northern regions of that vast empire, the same palæozoic divisions which have been proposed as types in the British Isles. During the last summer we extended our researches to the distant Ural, the Siberian plains, and the steppes of the south; and afterwards terminated the

these observations by a general transverse section from the
zof to the Baltic. Although we carried with us into Russia,
y be called the geological key of that great country, by which
subdivisions and relations of these rock masses have been
ed, let me say that Russia herself contains naturalists and
s who would rank high in any land. In palæontology,
d and Pander have already largely contributed to our know-
he first, by numerous local works, and recently by his illus-
of the Silurian strata in the Baltic provinces of Russia; the
y his very original researches into the fossils of the same
he lithological characters and detailed relations of which
t given by our own Strangways. Professor Asmus of Dörpat
to enrich us, as I have already stated, with a most curious
orate work on the fishes of the Old Red or Devonian system.
reat steps, however, which Russia is now making in field
and stratigraphical arrangement, are owing to the clear and
ned view of this subject which has recently been adopted
mperial School of Mines at the suggestion of the energetic
their staff, General Tcheffkine, who, under the orders of
ghtened Minister, Count Cancrine, has taken the surest
f advancing practical geology, and of rendering many
f his corps well acquainted with our subject; not only by
the suggestions of those qualified to judge respecting the
n of geological maps, but by so increasing the fossil collec-
the Imperial School of Mines, that it is now furnished with
ustrations of the sedimentary deposits of the empire, even
remotest parts of the Altai and the countries bordering on
It will be my duty and pleasure very shortly to bring before
ice the names of many officers of the Russian corps of Mines,
bours were of material use to myself and associates in our
xplorations; but I cannot resist naming at once Colonel Hel-
he inspector of the establishment, who whether he be viewed
ical geographer, a geologist, or as a writer, has rendered most
service to Russia by his luminous and attractive descriptions
ructure and outline of various parts of the empire, including
remote tracts. I beg also to refer you to the five published
of the School of Mines, as works containing much excellent
d highly creditable both to the government which pro-

moted their publication, and to the officers whose memoirs they contain.

In the mean time, besides what is doing on the Neva, a periodical work on Russia has appeared at Berlin under the title of 'Archiv für Wissentschaftliche Künde von Russland,' by the enterprising traveller A. Erman, of which two parts are published. Together with various memoirs on physical geography, history, language, antiquities, and physics, the editor has added a sketch of the recent advances in the geology of Russia, and illustrates his views by the publication of a small outline map of the empire. In the estimate of the geological steps in Russia which various labourers have accomplished, I rejoice to see the name of our countryman Strangways placed where it ought to be, as the first who applied the methods of modern practical geology to that empire, by the publication of his general map in the year 1822. Nevertheless it is too certain, as M. de Verneuil and myself informed you last year, that when we first visited St. Petersburgh in 1840, this map, though published in our Transactions, was as far as we could ascertain, unknown to the men of science in that country. In the first memoir on Russia, we specially directed your attention to the merits of Strangways, and we shall have ample opportunities hereafter of reverting to them. What I have now to observe in reference to the map of M. A. Erman is, that in his account of it, the special researches and the new points which my friend M. de Verneuil and myself established, are merged with what I must consider the copies of our views. The source whence the chief materials were obtained, is sufficiently proved indeed by the words "Silurische und Devonische schichten" engraved upon the map, particularly when coupled with the fact, that M. de Verneuil, Count Keyserling, and myself are the *only* geologists who traced the older groups to the White Sea, aided materially, as we have previously acknowledged, in a part of that region, by the Baron A. de Meyendorf, and for a short time by Professor Blasius. The original observations which we made were inserted by myself on a map which was shown at Moscow and St. Petersburgh in August, and to the British Association at Glasgow, in September 1840. On this map the range of the great bands of Silurian, Devonian, and Carboniferous rocks from St. Petersburgh and Moscow to the White Sea, with a vast basin of red deposits in the governments of Vologda and the Middle Volga, were laid down,

I assert, *for the first time*, and thus established the essentially distinguishing features of subdivision of the North of Russia.

After the application of this basis, Colonel Helmersen, to whom I have alluded, put together in the ensuing winter a small general map of Russia in Europe, in which he inserted the result of the labours of M. de Verneuil, the Baron A. de Meyendorf, Count Keyserling, Professor Blasius, and myself, acknowledging our services as well as those of all previous observers. The map of M. Erman which followed, was prepared by the Baron A. de Meyendorf and his companions, who extended the knowledge which they acquired with M. de Verneuil and myself to some of the central and southern parts of Russia, and thus marked a new step in the development of the structure of the empire. Since that time, the extended geological researches of the expedition in which my friends M. de Verneuil and Count Keyserling were associated with me, aided by Lieut. Koksharof, and an independent survey of Colonel Helmersen, have thrown a new light over the structure of various parts of the central, eastern, and southern regions, and have rendered necessary considerable changes in all previous maps. As a mere prelude, therefore, to what may hereafter appear, I have, with the aid of my associates, coloured a small general sketch-map of the empire, including the Ural chain, which as it will shortly appear before you in a published form, I only mention in this place to assure you that it differs very essentially from all previous maps.

Whilst on the topic of Russia, I will now state, that if on account of the preparation of this discourse and other official duties I had not been greatly occupied, I might before now have presented to you some of the results of the second visit to that country. In the mean time, however, my colleagues, M. de Verneuil and Count Keyserling, have been sedulously comparing our collections of fossils, and reducing a vast number of barometrical observations, whilst with their cooperation I have already completed a general table of superposition of Russian deposits, which, with a section across Russia, and the map above alluded to, are now nearly ready for publication. My brother geologists will feel that a general table of classification ought to be the finishing stroke in illustration of any country previously little known, and respecting which so much confusion prevailed. We offer it, however, in the persuasion that

its leading divisions will be supported by the evidences hereafter to be brought forward, and we simply put forth this table (which was drawn up at Moscow after our second journey) to convey to the cultivators of our science the chief results of our inquiries, and to place them upon record as bearing date from September 1841.

Among these results I will now merely allude to the first announcement of some of them, in a letter of the above date, addressed to Dr. Fischer de Waldheim at Moscow, in which the two points most dwelt upon were the discovery of a large central dome or axis of Devonian rocks, which separates Russia in Europe into two great north and south basins of very dissimilar characters; and the classification of certain cupriferous deposits of sand, marl, limestone, &c. under the term of "Permian system." As the explanation of the reasons which led to the suggestion of this name will be shortly offered to you in full detail, I should not now occupy your time by alluding to it, had not the mention of the word already called forth from M. A. Erman the remark, that these deposits have been long known to other observers. I admit that they were mineralogically known, but I deny that their geological position had been determined by any competent geologist previous to the researches of myself and friends; and I contend that there was no Russian formation concerning whose age so many contradictory opinions had been expressed. As a proof of this, I may state that the illustrious Humboldt himself assured me in the spring of last year, that it was the great point to which he hoped our labours would be directed. So strongly indeed was the difficulty of placing these strata in their correct geological horizon felt by Russian observers, that Major Wangenheim von Qualen, who had long and patiently studied them *in situ*, and Dr. Fischer, who had ably described many of their fossil contents, at once abandoned the field to my associates and myself, and put us in possession of all their knowledge, avowing their inability to arrive at a satisfactory geological conclusion. I was, therefore, surprised to read the premature criticism of M. A. Erman; the more so, as that author has called a large portion of the great limestone of Russia, *Jurassic*, which we have ascertained to be carboniferous, and to form the support of the hitherto anomalous system, which we shall endeavour to place in parallel with its equivalents in Germany and the British Isles, by showing its place in

the order of superposition, and by describing the fauna and flora by which it is characterized as a distinct type intermediate between the Carboniferous and Triassic systems.

FRENCH SCHOOL.

From the northern parts of Europe let us now pass to the consideration of the chief points of progress which our opposite neighbours are making. The publication of the splendid geological map of France, executed by Messieurs Elie de Beaumont and Dufrénoy, is indeed a subject of gratulation for the scientific men of all countries. Commenced in 1827, the map would have appeared five or six years earlier, had not the engraving of it led to unexpected delays. The part surveyed by each author is easily defined. France was divided by a line, proceeding from Hâvre, through Alençon, Avallon, Lyons, and Marseilles, to the Mediterranean. The western part was assigned to M. Dufrénoy, and the eastern to M. de Beaumont; but each was empowered to extend his observations, not only beyond the line of division, but also into those parts of the neighbouring countries which are included within the limits of the map.

The authors pursued their researches separately for several years, but as soon as they had settled the bases of classification they united to survey those points which required their conjoint examination, and by this means they finally established a perfect agreement in all the parts of their great undertaking. During the last five or six years, since the main features of the map were completed, the results have been communicated to every geologist who sought information, as I myself have experienced in my visits to Paris; and the authors, accepting in the mean time the contributions of others, have brought the map to its present degree of perfection.

Wishing to popularize geology in France, and to give their labours an extended sphere of usefulness, Messieurs de Beaumont and Dufrénoy have published, with the first volume of explanations which accompanies the large map, one on a reduced scale, giving an exact idea of the disposition of the mineral masses, and facilitating the comprehension of the admirable descriptive memoirs contained in the volume.

A desire has been often expressed, as you know, that all geolo-

gists might come to an understanding on the choice of colours, so that geological maps might be a sort of book written in a universal language. This idea, as our own great geological geographer Mr. Greenough has found, is more plausible in theory than practicable. In the selection of their colours, I confess, I regret that our foreign associates have not employed the normal colours used in the map of England, but then we must recollect that the principle of their colouring was decided and put into execution long before the publication of Mr. Greenough. The authors of the French work have however done well in giving one colour only to each great natural division of rocks, and they have distinguished the subdivisions by conventional signs, in a similar manner to that employed in the map of the Silurian region and Mr. Greenough's map of England. The advantage of this certain method of showing the relations which exist between the different parts of the same formation, is now thoroughly recognised.

Under the modest title of explanation of the map, the authors will publish three quarto volumes, of which the first only has yet appeared, and judging from this specimen we have a right to conclude that they will form one of the most splendid and useful works ever executed on the geology of a great country. In the introductory chapter of the published volume the general principles of the science are admirably given, and the succeeding chapters are occupied by descriptions of the "*Massif central de la France*: *Presqu'ile de Bretagne, Ardennes*: *Vosges*: *Montagnes littorales du Departement de Var*: *Terrains Houillers.*" The authors have divided their descriptions into great geographical regions, beginning with the most ancient formations; and I cannot resist expressing how much pleasure it has given me to see that these eminent men have adopted the divisions and nomenclature which have been proposed for the palæozoic rocks of England. In the other volumes the authors will describe the more recent formations, reserving for the conclusion, the account of those parts of France where the elevated and dislocated sedimentary deposits present problems most difficult of solution, and which continue to raise doubts in the minds of the best and most experienced observers. In their description of the rocks, the authors, faithful to what may be called the "natural method," have classed together all those which appear

to have a common origin, such as granites, porphyries, basalts, trachytes, &c.

In short, the geological map of France, and the volumes of explanation to accompany it, will form one of the finest monuments raised to science in our æra, and must be constantly consulted by those who wish to understand the spirit of that school of geology, which has cast such a brilliant light over France and throughout Europe. Doubly grateful indeed is the production of the work to ourselves, for in presenting it to this Society its authors have assured us that it was in our own islands they first acquired that knowledge of classification which led them to attempt the great enterprise, the completion of which so well sustains the high reputation they enjoy. Further, when we recollect that the knowledge of our foreign associates was one of the first fruits of the general peace, well may we now view the noble structure they have reared upon such a basis, as a convincing proof of the advantages conferred on science by the friendly intercourse of nations, which now rival each other only in advancing science and art!

Another most important work undertaken in France during the last year, is 'La Paléontologie Française' of M. Alcide d'Orbigny. Early initiated into the study of organic bodies and the anatomy of mollusks, this naturalist has acquired, during his extended travels, a good knowledge of positive geology; and he is therefore peculiarly qualified to carry into effect his arduous enterprize of describing the fossils of France in the order of the formations. He has commenced this vast undertaking by publishing during last year 139 plates, and upwards of 500 pages of text, on the Cephalopods of the Chalk. It is only necessary to glance over the figures, to perceive the care with which the different parts of the fossils are delineated. I particularly recommend to your notice the new genera, named by M. d'Orbigny "*Ancyloceras*" and "*Toxoceras*," and which added to the "*Crioceras*," recently introduced into the science, increase that infinite variety of forms in which the great Ammonite family expanded, previously to its total disappearance from the living world.

The Cephalopods, very rare in the upper beds of the chalk, occur in such prodigious quantities in the lower parts, and particularly in the "Neocomian" group, as defined by continental geologists,

thus they secure all the Numbers hitherto published of the "Paléontologie Française." The Ammonites have been the object of especial study to M. d'Orbigny, and have led him to conclusions of the highest interest, both zoological and geological. In the former respect, his observations on the external characters of Ammonites, and on the limits of their natural and accidental varieties; of the differences of sex, and particularly of age, are entirely original. Following these remains through the period of their development, he describes the transformations they undergo, and investigates the laws of such changes. The chambers, or the internal characteristics of Ammonites, the importance of which was long ago indicated by Von Buch, have presented new features to M. d'Orbigny, which are easily applied to the purposes of classification. I speak of the distinction of the "*selles et lobes en parties pleines et vuides*," according as they are cloven at the extremity, or terminate in a conical point. Combining this characteristic with that of the length of the dorsal lobe, and with those afforded by the exterior ornaments of the shell, the form of the back and mouth, between which there is almost always a coincidence, M. d'Orbigny has made twenty-one natural groups, of which eleven had been already established by Von Buch, and ten are new. Of these twenty-one groups, seven are peculiar to the Jurassic or Oolitic formations, ten to the cretaceous, and four contain species common to both.

M. d'Orbigny points out the modifications of species through time and space, and shows the relation that exists between certain forms and the beds which contain them. He recognizes three new creations or replacements of the species of Ammonites during the cretaceous period, and thus establishes, on zoological data, three divisions of natural groups;—first the Neocomian*, second, the Gault, and third, the Upper Greensand (Craie chloritée), and the White Chalk; and he estimates that in this triple succession of deposits, the Ammonites gradually decrease according to the numbers seventy-five, forty-two, and twenty-seven, to disappear finally with

* We have to learn why the very well-defined British formation, the Lower Greensand, seems to be suppressed and merged by our opposite neighbours in the "Système Néocomien." Cannot the Lower Greensand be preserved and the Neocomian be considered as a marine equivalent of the Wealden?

the uppermost chalk or Maestricht beds, and before the tertiary epoch.

The total number of determined species of Ammonites in the great cretaceous system of France is 144, according to M. d'Orbigny, and with the exception of three, which are common to the Gault and the Upper Greensand, all the other species are divisible into groups, each of which is peculiar to one of the three great divisions of this system, and may be considered characteristic of it. Although the species have been thus replaced several times during the cretaceous period, there exists, however, among them a certain affinity of forms which differs sufficiently from the general characteristics of the Jurassic Ammonites to constitute the beds containing them a truly distinct and separate series. We may congratulate M. d'Orbigny on having begun his 'Palæontology' with the fossils of this period: for whilst the labours of the English, particularly the admirable general views and detailed descriptions of Dr. Fitton, and the works of Dr. Mantell, have contributed to a good acquaintance with the northern chalk and greensand, it must be confessed that there is ample room for research in the southern type.

In consequence of the numbers of fossils sent to M. d'Orbigny from all parts of France, and which I had the pleasure last spring of seeing on his tables, a new light may be thrown by the 'Paléontologie Française' on the classification of the sedimentary masses of the Alps and Apennines; the limestones of Greece, Turkey in Europe, Palestine, the coasts of Africa and in fact of the whole circuit of the Mediterranean, the chief formations of which are at present arranged in the cretaceous epoch.

I might now notice the recent labours of M. Rozet, M. Leymerie, M. Rolland du Roquand, M. Duval and others, whose memoirs have been partly published in the volumes of the Geological Society of France, but such duties pertain to the office of the President of the French Society, and doubtless, the eminent man* who is now at the head of it will do ample justice to these authors.

BELGIAN SCHOOL.

In Belgium, the most important works that claim our attention for the year 1841, are,—1st, the completion of the field survey of the

* M. Cordier.

and of a series of organic remains presented to the Society, the views respecting these strata entertained by American geologists; and the second on parts of New Brunswick, particularly the coal-measures which extend over a wide area, and rest in some places upon granite and in others upon schistose rocks; and he shows that though granite veins penetrate the slate, not one is to be found in the coal-measures: hence he infers, that the schists are the oldest rocks of the country, and the coal measures the newest.

THE GLACIAL THEORY.

The last subject I will advert to is that of glacial action, which has recently occupied the thoughts of many geologists. From a study of the Alps, where Venetz and Charpentier led the way in showing that a connexion existed between the erratic blocks and the advance of glaciers, Professor Agassiz has deduced a glacial theory, and has endeavoured to generalize and apply it even to our own countries, in which effort he has been supported by my predecessor in the Chair. In the following observations, I will endeavour to point out what new materials have been brought forward, abroad and at home, to enable us to reason correctly on this difficult question, and I will then suggest some essential modifications of the new hypothesis.

As propounded by Agassiz, the glacial theory, even in its application to the Alps, has met with an opponent in the person of Professor Necker de Saussure. In the first volume of a work which he is now publishing, M. Necker treats in great detail the whole subject of superficial detritus connected with the northern and western watersheds of the Alps, and gives us the fruits of many years of observation. [illegible]

the flanking and lower mountains, and even the strata on which it rests, having contributed comparatively little to the great advancing body. Examining the high valleys about Chamouni and the foot of Mont Blanc, and finding massive walls from 300 to near 600 feet in height, composed of this ancient diluvium in its coarsest form, near the extremities of certain glaciers, he concludes that they were once the moraines of glaciers which melted away and retired from them. He then goes on to suppose that when the recession of the glaciers took place (an effect which he refers to the same cause as De Saussure) such transversal moraines formed dykes standing out at some distance from the mountain and barred up lakes formed by the melting of the snow and ice. These lakes, at length swollen to excess, are supposed to have burst through the moraine barrier, and to have drifted the materials of which it was composed into the lower countries. M. Necker believes that when these ancient glaciers existed, the Alps were considerably higher than at present, and he judges that such was the case, because the "aiguilles" of Mont Blanc have been lowered very considerably in our own times. Arguing that great blocks are never found at the foot of mountain chains which have not permanent glaciers, of what De Saussure called the "first class," he cites many negative examples, and brings forward the Pyrenees, where no true erratic blocks are seen, as a proof that the minor or second class glaciers, which there occur, never advanced sufficiently far to dam up water-courses, and thus to form those great lakes, to the letting off of which and to the destruction of vast moraines, he attributes the presence of large boulders in the Alps.

I must, however, remind M. Necker, that if he assumes that all great erratic blocks are to be referred to some *neighbouring* chain, now the seat of glaciers, he forgets the cases in Scotland and England, and indeed many others, far removed from mountain ranges, and which must be classed, as I shall presently show, with submarine deposits. Indeed by far the widest spread of erratic blocks with which we are acquainted, extending over the plains of Germany and Russia, must have taken place (as I believe at least) when those flat regions were beneath the sea, for recent observations have shown, that the blocks constitute the uppermost or last surface deposit in tracts which exhibit, here and there, proofs of having been an ancient

aid of a series of organic remains presented to the Society, the views respecting those strata entertained by American geologists; and the second on parts of New Brunswick, particularly the coal-measures which extend over a wide area, and rest in some places upon granite and in others upon schistose rocks; and he shows that though granite veins penetrate the slate, not one is to be found in the coal-measures: hence he infers, that the schists are the oldest rocks of the country, and the coal-measures the newest.

THE GLACIAL THEORY.

The last subject I will advert to is that of glacial action, which has recently occupied the thoughts of many geologists. From a study of the Alps, where Venetz and Charpentier led the way in showing that a connexion existed between the erratic blocks and the advance of glaciers, Professor Agassiz has deduced a glacial theory, and has endeavoured to generalize and apply it even to our own countries, in which effort he has been supported by my predecessor in the Chair. In the following observations, I will endeavour to point out what new materials have been brought forward, abroad and at home, to enable us to reason correctly on this difficult question, and I will then suggest some essential modifications of the new hypothesis.

As propounded by Agassiz, the glacial theory, even in its application to the Alps, has met with an opponent in the person of Professor Necker de Saussure. In the first volume of a work which he is now publishing, M. Necker treats, in great detail, the whole subject of superficial detritus connected with the northern and western watershed of the Alps, and gives us the fruits of many years of observation. Adding very considerably to the list of phœnomena of transported materials collected by M. A. de Luc, he takes his own illustrious ancestor, De Saussure, as his model, and following in the track of the historian of the Alps, he endeavours to enlarge and improve upon that great observer's suggestions. Pointing out the distinctions between two classes of detritus, viz. one of high antiquity and another of modern date, M. Necker contends that the enormous masses of the ancient drift or diluvial detritus have a direct connexion with the actual configuration of the surface, because the *chief* part of them has been derived from the centre of the chain,

bottom of a sea. But without extending his theory to other parts of the world, it does not appear to me, even when confined to the Alps, that M. Necker explains satisfactorily how the granite blocks of Mont Blanc should lie upon the Jura, by any reference to subaërial debacle; for if we are to imagine the deep hollow of the lake of Geneva, filled up with gravel, sand and mud, and forming an inclined talus from the centre to the flanks of the chain, the subsequent scooping out of this enormous mass of materials involves an intensity of degradation as difficult to believe in as the former extreme climate of Agassiz, by which thousands of feet of snow and ice are supposed to have occupied the same deep valley. I ought not to omit to state that one of the chief elements introduced by Agassiz into this question, the polished and striated surfaces of the rocks, has not yet been alluded to by this author, but will be treated of in his second volume.

In the mean time, however he may fail to account satisfactorily for the transport of the very distant great blocks, we have to thank M. Necker for the additional materials, which seem to establish one fundamental fact in reference to the Alpine case, viz. when this detritus was cast off, the gorges and flanks of the chain had nearly the same reference to the central crest as that which now prevails. If this be proved, the theory which depends chiefly upon the supposition, that a great elevation of the centre of the chain broke off the ice and dislodged the glaciers, is deprived of its chief basis. In what manner Professor Agassiz can account for the Alps being a great centre of dispersion *when at a lower level*, is indeed a part of his theory which is not easily comprehended. On the other hand, whatever we may think of M. Necker's hypothesis, it must be admitted that the facts adduced by him support one essential point of the glacialists, by connecting the presence of blocks with the existence of glaciers in the Alps, the former being, as he states, invariably found both in the southern and northern watersheds of those mountains, and at the mouths of the great transverse ravines which lead up to the regions of perpetual snow, and in all such cases he allows that the condition of the blocks is highly indicative of their having once formed part of the "moraines" produced by former glaciers.

But the important point, that the glacier is the chief source of

the origin of erratic blocks, is entirely denied by another antagonist to the theory of Agassiz, who has appeared in the person of M. Godeffroy *.

After the observations of two summers in the Alps, this author has become convinced that the materials of the so-called moraines have not been derived simply by the glacier from the solid rock in the higher mountains, but are the re-arranged portions only of a great pre-existing diluvial deposit, which had been accumulated in the radiating valleys during a period of great disturbance, anterior to the existence of glaciers in that latitude. Describing (like M. Necker) one of these "traînées" as having a continuous length of fifteen leagues, he infers that such a mass could never have been deposited by a glacier proceeding from mountains of no greater altitude than the Alps. Arguing that glaciers are merely the condensed or central portions of vast accumulations of snow, forced downwards into the gorges by increasing volume from above, the chief novelty of M. Godeffroy's work is contained in the opinion, that in advancing, these bodies of ice cut through the ancient diluvium or drift, just as a plough-share cleaves the soil ("presso tellus consurgit aratro" being his motto), and threw up some portions into lateral moraines, as well as pressed before them others to form terminal moraines. To the crystalline and mechanical changes which the snow has undergone in its passage into solid ice, is attributed much of the confusion and irregularity of outline so visible in the "aiguilles" and other icy masses of the Alps; and to the same disturbing action is referred the rounded and worn exterior of the boulders in moraines, as contrasted with comparatively angular blocks of the pre-existing drift which have not been in contact with the glacier. I refer you to the work of M. Godeffroy for the explanation of the manner in which he supposes the surface of the advancing or retreating glacier was subjected to lateral overflows or "écroulemens" of stones, gravel, and earth, and also for his theory of medial moraines; but I now bring to your notice his ingenious effort to solve one of the very difficult climatological problems in the Alps. Having shown how the lower valleys must, from year to year, become more and more encumbered with detritus, he seizes this fact to explain

* Notice sur les Glaciers, les Moraines et les Blocs Erratiques, 1840.

by it alone, both the well-known retreat of the glaciers and the fact brought forward by Venetz and other observers; viz. that roads which existed in certain former passes of the High Alps are now quite choked up with snow and ice—a fact which has been supposed to indicate a sensible decrease of temperature within the historic æra. M. Godeffroy contends, that in ancient times, when the gorges were more open, and the heaps of detritus at the entrance into the lower valleys were less in size and fewer in number, and when consequently the glaciers easily extended to greater distances, the continual and unrestricted supply of snow and ice from many affluents more than countervailed the loss through atmospheric action; but that as the obstacles increased at some distance above the terminal moraine, the lower ends of the glaciers not being so fed as to regain in one season the melting losses of the previous year, the inevitable result was a successive shrinkage and retrocession of the mass. The increase of snow and ice in the upper passes, and the blocking up of the roads, are explained by the same agency; for as soon as the descent of the glacier from the higher to the lower Alps was impeded, it would follow, that the frozen matter of the higher regions, deprived of its previous exit, must find its way into the adjacent upper depressions, and there form those *mers de glace* which have obstructed the road-ways or passes of our ancestors. Thus is the supposed anomaly explained without recurring to any change of climate*.

In that part of our own country to which the glacial theory has been applied, Mr. Charles Maclaren, already known to you by excellent geological treatises, has recently published a well-condensed, small work explaining the views of Agassiz. The phænomena of glaciers and the general doctrines derived from their study being explained, Mr. Maclaren proceeds to analyze those cases of trans-

* I hoped to have been able to quote the opinions of Professor J. Forbes on this *vexata quæstio*, because it is well known that he was a companion of Professor Agassiz in the Alps during the last summer, but this distinguished cultivator of physical science has not yet published his views on the action of glaciers as affecting the surface of the earth, though he has given to the public a very ingenious sketch, descriptive of a peculiar parallel striation in the solid ice of glaciers.—Edinburgh New Philosophical Journal, January, 1842.

ported detritus in the neighbourhood of Edinburgh to which the theory had been supposed to apply.

A year and a half only has elapsed since Professor Agassiz and Dr. Buckland seemed to think, that this district was as rich in proofs of the action of glaciers as many other parts of Scotland which they visited, and as I happened to witness the efforts of my predecessor in this Chair to attach Mr. Maclaren to his views, I must be permitted to direct your attention to the practical results at which this gentleman has arrived, in some prominent cases.

Observing blocks of greenstone on Arthur's Seat, which, from their peculiar structure, must have been transported from Salisbury Craigs, a *lower* hill, and separated from the former by an abrupt valley, Mr. Maclaren infers, that if the present surface of the land be argued upon (and in all questions of glaciers this is a postulate), neither glacier, nor iceberg, nor current will explain the fact. It is unnecessary that I should here examine this author's hypothesis, by which in order to solve the local problem, he restores the inclined stratified masses of Salisbury Craigs to such an extent as to give them an altitude in ancient times superior to that of Arthur's Seat; for whether we adopt his ingenious view, involving a mighty subsequent denudation, or suppose that in the oscillations of this plutonic tract the former low and high points of land have been relatively depressed and elevated, it is obvious, from the very structure of the rocks, that in both cases a subaqueous, and not a subaërial condition is called for to explain the appearances, and this too, be it recollected, on the summits of the highest hills in the immediate vicinity of the Scottish metropolis, in and around which the action of glaciers has been supposed to be visible at much lower levels!

Among the examples of the scratched and polished surfaces of rocks near Edinburgh, I do not perceive that the glacialists have grappled with certain appearances on which Dr. Buckland formerly dwelt with so much pleasure, viz. the grooved or channeled surfaces of the Braid Hills, first pointed out by Sir James Hall, and which the great chemical geologist attributed to a powerful rush of waters. When I visited the low ridge in question with Dr. Buckland and other friends*, my conviction was that these grooves,

* Dr. Graham and Mr. Maclaren were of the party, in Oct. 1840.

though then attributed by Dr. Buckland to glacial action, are due neither to that agency, nor to any rush of waters, but are simply the result of the changes which the mass of the rock underwent, when it passed from its former molten or pasty condition into a solid state. These appearances differ essentially from ordinary glacial scratches or scorings*. They are, in fact, broad undulations or furrows, and instead of trending *from* the higher grounds to the Firth of Forth, as would naturally be the case if they were due to the expansion and descent of glaciers, they rise up to the very *summit* of the low ridge in a direction transverse to its bearing, and with no neighbouring point of ground higher than that on which they occur. On clearing away the thin turf which barely covered the rock, some of these undulations in the surface appeared wide enough to contain the body of a man, and though observing a rude sort of parallelism, their forms were often devious. As their surface was smooth, not much unlike the usual aspect of the so called "moutonnés" rocks, the glacialists of our party at first seemed to be proving their case, when suddenly a discovery destroyed, at least in my opinion, their theory; for in the adjacent quarries of the same hill, at a much lower level, and upon beds just uncovered by the workmen from beneath much solid stone, other sets of undulations or grooves were detected, so like to those upon the summit of the hill, that a little atmospheric influence alone was required to complete their identity. My belief therefore is, that the undulations were caused by the action which took place when the stone was solidified.

Phænomena of a similar nature to the Scottish have been since observed in Wales by our late Fellow, Mr. Bowman. Captivated by the glacial theory, and having himself endeavoured to show that it could even be as successfully applied to the South as to the North of Scotland, he examined the highest region of Wales, with the geological structure of which he was previously familiar, half convinced, *à priori*, that he would naturally find in those mountainous tracts some proof in support of the new views which he had adopted. He, however, quitted that country without having been able to observe any evidence whatever in favour of the Alpine theory.

* Plaster casts of these exist in the Geological Society.

though his journey enabled him to detect several examples of striated rocks, which in unskilful hands might have been mistaken for the effects of glacial action; and these he holds up as warning beacons. After stating that there are, in his opinion, no terraces which any follower of Agassiz can construe into "moraines," whether terminal, medial, or lateral, on the flanks of the mountains of Snowdon, the Arenigs, or the Berwyns, he describes three distinct and differently formed sets of parallel markings which he observed in the newly uncovered surfaces of the schistose Silurian rocks, and shows satisfactorily how such appearances, as well as the tops of the joints, might be mistaken by cursory observers for scratches, although they are in fact due to structure.

Unlike Mr. Bowman, Dr. Buckland has not confined his views of the action of glaciers to Scotland, but applies them largely to the North of England and to Wales. He has recently endeavoured to satisfy us, that the rocks on the sides of the chief valleys in the latter country which open out from a common centre of elevation are striated, worn and polished in the direction of the present watercourses, and these he conceives to be evidences of former *glaciers*, which filled up all the valleys radiating from Snowdon to a distance of many miles from a common centre. I confess I see almost insurmountable objections to this view. Apart from other evidence, the very physical geography of this tract is at variance with the construction of such an hypothesis. In the Alps, and indeed in every other part of the world in which they have been observed, the length of glaciers is in ratio to the height of the mountains from which they advance, or, to use the words of Agassiz, from which they *expand*. Now whilst in the present days, a small glacier hangs to the sides of a mighty giant like Mont Blanc, having the altitude of 15,000 feet, our Welch hills, having a height only of 4000 feet, had glaciers, by the showing of Dr. Buckland, of a length of many miles. Again, in the same memoir, which fills so large a portion of the principality with glaciers, the author comments upon certain facts already well known to us, viz. the existence upon Moel Tryfane and the adjacent Welch mountains of sea shells of existing species, at heights of 1500 and 1700 feet above the sea, where they are associated with mixed detritus of rocks transported from afar, all of which have travelled from the North, the hard chalk and flints of the North of

Ireland being included. How are we to reconcile these facts with the theory that the greater part of the country in question was frozen up under the *atmosphere* in some part of the same modern period? Unable otherwise to explain how marine shells should be found on mountains which are supposed to have been previously and during the same great period occupied by terrestrial glaciers the accumulation of ages, Dr. Buckland invokes anew the aid of the old hypothesis of a great *wave*. This wave, rolling from the north, must have dashed over the mountains to a height of near 2000 feet, depositing as it went gravel, boulders and fragments, derived from places 200 miles distant, and transporting also marine shells in its passage. But is it not more natural and accordant with all the data upon which our science has been reared, to suppose that when such shells were deposited, the parts of the mountains so affected were permanently beneath the sea, than to call into play the assumption of the passage of so mighty a wave? At one moment the argument used is, that scratches and polishings of rock must have been done by ice, because in existing nature it has been found that ice can produce such effects; and in the same breath we are told that beds of shells have been placed on a mountain by an agency which is truly supernatural.

In fact, the "glacier" theory, as *extended* by its author in proving too much, may be said to destroy itself. Let it be limited to such effects as are fairly deducible from the Alpine phænomena so clearly described by Agassiz, and we must all admire in it a *vera causa* of exceeding interest; but once pass the bounds of legitimate induction from that *vera causa*, and try to force the many and highly diversified superficial phænomena of the surface of the globe, into direct agreement with evidences of the action of ice under the atmosphere, and you will be driven forward, like the ingenious author of the theory, so to apply it to vast tracts of the globe, as in the end to conduct you to the belief, that not only both Northern and Southern hemispheres, but even *quasi* tropical regions, were shut up during a long period in an icy mantle. Once grant to Agassiz that his deepest valleys of Switzerland, such as the enormous chasm of the lake of Geneva, were formerly filled with solid snow and ice, and I see no stopping-place. From that hypothesis you may proceed to fill the Baltic and Northern Seas, cover Southern

England, and half of Germany and Russia with similar icy sheets, on the surfaces of which all the northern boulders might have been shot off. But even were such hypotheses granted, without we also build up former mountains of infinitely greater altitude than any which now exist, we have no adequate centres for the construction of enormous glaciers which imagination must create in many regions to account for the phænomena. The very idea which records the existence of these vast former sheets of ice is at variance with all that is most valuable in the works of Charpentier, Venetz, and Agassiz, whose data, as carefully eliminated from Alpine phænomena alone, would naturally teach us never to extend their application when those conditions are absent, viz. the mountain chain, by the very presence of which the phænomena are explained.

But though the Alpine glacial theory be new, the scratches and polished surfaces of rocks are by no means of recent observation. Many Swedish miners, from the days of Tilas and Bergman, failed not to remark how their mountain sides were furrowed, and in our own times, Sefström* of Sweden, and Böhtlingk of Russia, have not only narrowly traced them over wide regions, but have endeavoured to account for them. The first of these authors remarked that nearly all the hard rocks of this country had a "worn or weather side," and a highly escarped or "lee side," the former being exposed to the North and the latter to the South; and having further shown that the detritus had generally been carried from N. to S., he called the worn face the "weather side," and the higher and jagged extremity of such ridges the "lee side." Extending his observations to many hundred places, he divided these scratches into what he calls normal and side furrows, showing that in the latter there are frequent aberrations from the persistent courses of the former. Although he had been at first disposed to think, from the data in a given country around Falun, that the normal lines were invariably from N. to S., he afterwards discovered that in large tracts of the South of Sweden the direction was from N.W. to S.E., and in others, particularly along the coasts of Norway, from N.E. to S.W.; all these facts being recorded on a map, which is a most valuable document.

Since Sefström's work was published, M. Böhtlingk, a young

* See Taylor's Scientific Memoirs, vol. iii. p. 81.

Russian naturalist of great promise, but, alas! prematurely carried to the grave, extended his researches to the northern territories of Russia. Observing that the dominant direction of the scratches in parts of the governments of Olonetz and Archangel was from N. to S., and that along the edges of the Bothnian Gulf their course was from W. to E., he passed the summit level of Russian Lapland, and found that there the drift had no longer been transported from N. to S., or from N.W. to S.E., but, on the contrary, from S.E. to N.W.; or, in other words, that the blocks of Lapland had been carried northwards into the shores of the Polar Sea. In a recent letter to Mr. Lyell, read before this Society, Professor Nordenskiöld has accurately recorded the phænomena of this class observed by him in Finland, and he shows that there the blocks and striæ proceed from N.N.W. to S.S.E.

The theory of Sefström and his followers is, that a great flood, transporting gravel, sand and boulders, was impelled from the north over pre-existing land, and that the deviations from the N. and S. direction are due only to various promontories by which the flood was deflected. So convinced was this author that with local aberrations all the transport throughout the whole of Europe had taken place from north to south, that he not only travelled over the whole of Germany and saw nothing except materials streaming in the same direction, but even carried with him his northern drift into the Austrian and Bavarian Alps. I will not waste your time by pointing out the errors into which his hypothesis, though founded on data good within a limited radius, led this author. Every one who has studied the Alps (and the facts were well known before the days of glacial theories), is perfectly aware that the detritus [illegible] nks has been shot off eccentrically from the higher central mass[illegible] observations indeed of Böhtlingk give the same result [illegible] rand scale in the North, and explain what his valuable labour, had left unknown, viz. that [illegible] mountains, as a whole, had produced exactly the [illegible]t as the Alps, having poured off their detritus in [illegible]*om a common centre*, the northern chain differing only [illegible]entral Europe by the much wider range to which its [illegible]oulders were transmitted.

[illegible] belief, Gentlemen, as you know, has been, that by far

the greatest quantity of boulders, gravel, and clay distributed over our plains and occupying the sides of our estuaries and river banks, was accumulated *beneath the waters* of former days. Throughout large tracts of England we can demonstrate this to have been the case by the collocation of marine shells of existing species with far-transported materials. It was the association of these testacea with foreign blocks in the central counties of England which first led me to attach a new and substantial value to that view of glacial action which had been so well advocated by Mr. Lyell before Professor Agassiz came forward with his great terrestrial and general theory. I am bound to say that wide researches during the last two years have strongly confirmed my early views*. I could not travel in the autumn of the year 1840 around the shores of the highlands of Scotland, without being convinced that the terrace upon terrace, presented on the sides of some of the great valleys, and often high upon the sea-ward hills of the bays opening out to the ocean, were nothing more than the bottoms of former seas and estuaries which had been successively desiccated.

I coincide, therefore, entirely with Mr. C. Darwin in his very ingenious explanation of the probable formation of the parallel roads of Glen Roy (Phil. Trans., 1839, p. 39). Since then that excellent observer has borne out similar views in a paper read before our own Society. In this memoir, estimating the different changes of the sea and land, and showing to what extent the solid strata were depressed, whose relative histories he thus reads off, he traces the shingle beds from the edge of the sea, where they are in process of formation, to considerable heights inland; and estimating how blocks were transported from the great Cordillera within, or not long before the period of existing sea shells, he explains the far-transported boulders by their being carried to the spots where they lie in vessels of ice. The melting of these icebergs he conceives to have been the principal agent in forming such masses of clay, gravel, and [illegible] while fusion [illegible] to be [illegible] formerly [illegible]

* [illegible]

ing agent he attributes the general absence of organic remains in these deposits; and, lastly, he infers that it is much more probable that the great boulders were transported in icebergs detached from glaciers on the coast, than imbedded in masses of ice produced by the freezing of the sea.

M. de Verneuil and myself had previously brought before you some new results, arising from our first expedition to Russia. We endeavoured to show the utter inapplicability of the Alpine glacial theory to vast regions of Northern Russia, though the surfaces of the rocks are scored and polished, and far-travelled blocks occur throughout a wide area in isolated groups, because much of this detritus has travelled over extensive tracts of low country, from which it has ascended to levels higher than the sources of its origin. Hence we inferred, that the onward persistent march (in many parts up-hill) of a body of glaciers, having a front of many hundred miles in extent, is irreconcileable with any imaginable subaërial action. On the other hand, it was proved, by the presence of sea shells of an arctic character, that the "terra firma" to which some of the blocks had been transported, had been the bed of the Northern or glacial Sea at the period of this transport. We then attempted to explain how the parallel striæ and polishing of the surface of rocks of unequal altitudes was reconcileable with the *submarine* action of ice, by supposing that ice floes and their detritus might be set in motion by the elevation of the Scandinavian continent, and the consequent breaking up of great glaciers on the northern shores of a sea which then covered all the flat regions of Russia; and we further stated our belief, that the bottoms of these icebergs, extending to great depths, must have every here and there stranded upon the highest and most uneven points of the bottom of the sea into which they floated; that where the bottom was hard rock, the lower surface of the iceberg, like the lower surface of a glacier, would grate along and score and polish the subjacent mass; that where the bottom consisted of tenacious mud or clay, the iceberg once fairly stranded would be retained till it melted away, entirely or in part, whilst it would be more frequently borne over sand-banks, on account of their less resistance. In this manner, we endeavoured to explain not only the scratches and polish of hard submarine rocks, but also why large blocks are often found on former

conceive why so few shells have been discovered in this coarse detritus, whilst we readily perceive why the stones impacted in it should be scored and striated, and often polished.

Besides the great advancement of our knowledge of terrestrial magnetism, which at some future day may be connected with our labours, the Antarctic expedition, under the distinguished navigator Captain James Ross, has, as might have been expected, thrown considerable light upon the glacial theory. A few years only have passed since the existence of an enormous mass of ice-clad land in the antarctic region, was announced by an American squadron of geographical research. This great icy tract, which was described as exhibiting hills and valleys, and even rocks upon its surface, has entirely disappeared in the short intervening time; for Captain Ross has sailed completely through the parallels of latitude and in the same longitude which it was said to occupy. As we cannot suppose that the American navigators were deceived by atmospheric phænomena, so must we believe that what they took for solid land, was one of the enormous accumulations of ice called "packs," the great source of those numerous ice islands, which periodically encumber the Southern Seas.

Continuing his progress towards the South Pole in almost open sea, Captain J. Ross discovered, as he proudly says, "for the honour of England," the southernmost known land, which he named Victoria, and which he coasted for more than eight degrees of latitude. This land rises in lofty mountain peaks, from 9000 to 12,000 feet in height, perfectly covered with eternal snow, from which glaciers descend, and project many miles into the ocean, te[illegible]g in perpendicular lofty cliffs. The rocks which c[illegible]amined were of igneous origin, and near the extr[illegible]. of his exploration, or in S. lat. 77° 3[illegible] [illegible]cent volcano was seen in full actior[illegible]. at an altitude of 12,400 feet. Furt[illegible]ard was then impeded by an enormo[illegible]ers 150 feet high, which stretched f[illegible]. which the bold seaman traced [illegible]ong. E. 191° 23′, and lat. S. 78°. [illegible]acier was inferred from the existence [illegible]untains behind it, the tops of which, [illegible]were estimated to be a degree of lat[illegible]ude

to the south of the sea-face of this great wall of ice, at not more than half a mile from which the soundings were at 318 fathoms deep, and upon a bed of blue soft mud. Here then the geologist is presented with abundant matter for speculation. Volcanos in the midst of eternal polar snow and glaciers, with seaward faces as wide as some of the continental tracts, which, from the striæ and polish on their surface, and the wide dispersion of blocks and detritus, are supposed to have been affected by former terrestrial glacial action. Whilst, however, we have here the proof that existing glaciers advance some few miles into the sea, we are also informed that the ice ceases suddenly against an ocean 2000 feet deep, and thus we are led to conclude that many glaciers, which may formerly have extended themselves into the sea, had a length, the extent of which, whether like this antarctic example, or those which have been measured in the Alps, was proportioned to the altitude of the ancient mountains against which they rested. By the same reasoning we may infer that the striæ and polish of rocks, or accumulation of coarse detritus, and large blocks which are only to be observed in places far beyond the limits that are now established between mountains and their dependent masses of ice, cannot be due to the advance of former solid glaciers, but must rather be referred, as I have argued, to the floating away of vast packs and icebergs liberated from *centres of congelation*.

But besides the submarine operations now in action, and which may serve to explain most of our ancient phænomena, it has been shown that in Russia and other cold countries there are several actual subaërial processes, by which large blocks are accumulated at different heights by the expansion of the ice of rivers, or have been piled up by the glacial action of former lakes, when at much higher levels *, leaving lines of coarse angular blocks.

I desist, however, in this place from entering further into the many features under which the existing agency of ice may be viewed apart from the results of the movements of glaciers. More than enough has indeed already been said: for so long as the greater number of practical geologists of Europe are opposed to the wide extension of a terrestrial glacial theory, there can be little risk that such doc-

* Geological Proceedings, Murchison and De Verneuil on Russia, vol. iii. p. 406.

trine should take too deep a hold of the mind. But whilst we may have no fear of this sort in Europe, I have lately read with regret certain passages in the Anniversary Discourse of Professor Hitchcock of the United States. In North America, striated, scored, and polished surfaces of rocks, proceeding from N. to S. for vast distances, occupy, it appears, at intervals a breadth of 2000 miles, and are seen on hard rocks at all levels from the sea-shore to heights of 3000 and 4000 feet. Professor Hitchcock tells us, that these phænomena and the accumulations of gravel and blocks had always been inexplicable, until the work of Agassiz unexpectedly threw a flood of light upon his mind*. If Professor Hitchcock could demonstrate what he now seems to believe, that the great mass of the continent of North America was formerly covered with ice, he must first prove that it was not at that period below the level of the sea; but as yet no facts are before us to lead us to doubt that the great accumulation of detritus and the transport of blocks did take place beneath the waters in that country. In justice, however, to this author, it must be said, that in expounding the glacial theory he ingenuously acknowledges the great difficulty of believing that solid masses of ice 3000 to 4000 feet thick, covered the whole region; that no action of a glacier will explain the persistent striation of the surface of an entire *continent* from N. to S., and that the direction of the boulders and the striæ is to a great extent up-hill. When these and many other difficulties shall have been carefully weighed, our transatlantic friends may be disposed to modify their views, particularly when they find that the existence of glaciers in Scotland and England (I mean in the Alpine sense) are not yet, at all events, established to the satisfaction of what I believe to be by far the greater number of British geologists.

The presence of Mr. Lyell at this time in North America, is indeed, most opportune, for whatever changes his mind may have recently undergone, no geologist has more strenuously laboured to

* Anniversary Address. Philadelphia, April 1841, p. 24. I must be excused for stating that Professor Hitchcock has entirely misconceived my views, when he places my name among those who had espoused the Alpine glacial theory. My efforts have been invariably directed towards its limitation, nay, to its entire rejection as applicable to by far the largest portions of the surface of the globe.

make himself master of all its bearings, or more systematically enlarged our knowledge of this disputed subject. Possessing as he now does the advantage of observation on a vast scale, I have little doubt that he will account for the wide dispersion of blocks in America from N. to S. by referring to a cause quite as general and quite as aqueous as that by which he originally sought to explain the phænomenon in Europe*.

Although the consideration of this subject has already carried me beyond the limits I had prescribed to myself, yet I cannot quit it without reminding you, that the greatest geological authorities on the continent, led on by Von Buch who has so long studied these phænomena in his native land, are opponents to the views of Agassiz. Even whilst I write, I find that M. de Beaumont has just communicated to the Institute of France, a report on the results of a journey through Lapland, Finland, and the north of Europe, by his countryman M. Durocher, in which grouping the facts with great perspicuity, he handles the whole subject with his usual master's hand, and points out the value of the previous observations of Von Buch, Brongniart, and other writers. M. Durocher conceives that the phænomenon of the transport of erratic matters has proceeded from two successive and distinct operations: the first a great current from the pole, to which the striæ and polish of rocks, and the deposits called Osars are referred; the second, the transport of the distant blocks by vessels of ice, when all that part of Europe which they cover was subjected to the immersion of an icy sea. He does not agree with M. Böhtlingk, that the point of departure of the current can be placed in Lapland, but supposes it to have proceeded directly athwart those regions from the pole†. But the point to which I

* See Principles of Geology, 2nd ed. vol. i. p. 342; and Elements of Geology, 1st ed. p. 136.

† M. Durocher has made two valuable observations in showing us that the striated and polished surface of the hard rocks is sometimes covered by accumulations of sand and detritus; and that although proceeding in a general sense from the north, the furthest transported blocks are so distributed as to indicate *radiation* from certain mineralogical centres, much in the same way as our blocks of Shap granite have, on a less scale, been scattered from one point of distribution. In stating, however, that in the progress of these transported masses to the south, granitic blocks always constitute the outermost zone, it appears to me that M. Durocher has ge-

now specially advert is, that in his skilful analysis of this memoir our eminent foreign associate admits floating ice as a *vera causa* to explain the drift of blocks, just in the same manner as in common with Lyell, Darwin, and others, I have been endeavouring to explain the phænomenon during the last three years, and thus the inference which was drawn from plain facts is admitted, viz. that the chief tracts covered by erratic blocks were *under the sea* at the period of their dispersion. (Sil. Syst. p. 536.)

Thus far had I written, Gentlemen,—in short I had, as I thought, exhausted the glacial subject at all events for this year,—when two most important documents were put into my hands. The first of these is the discourse of my predecessor, who has so modified his first views, that I cannot but heartily congratulate the Society on the results at which he has now arrived. . I rejoice in the prudence of my friend, who has not permitted the arguments of the able advocate to appear as the sober judgment of so distinguished a President of the Geological Society. In fact, it is now plain that Dr. Buckland abandons, to a great extent, the theory of Agassiz, and admits fully the effects of water as well as of ice, to account for many of the long-disputed phænomena. Whilst this admission involves the concession for which we have been contending, viz. that

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neralized beyond the field of his own observation. In Russia, for example, M. de Verneuil and myself traced greenstone blocks to the same southerly latitudes as granites. The blocks between Jurievitz and Nijny Novogorod are composed of quartz rock and of the peculiar trappæan breccia known in Russia as "Solomenskoi-kamen," the parent rocks of which we examined *in situ* near Petrazowodsk (Geol. Proceedings, vol. iii. p. 405), whilst the extreme boundary of these boulders extends to Garbàtof on the Okka, S.W. of Nijny Novogorod, and consequently very far beyond Kostroma, the limit assigned to them by M. Durocher. Again, if M. Durocher prolongs the northern drift to the flanks of the Ural Mountains he is decidedly in error, for there is no coarse detritus whatever on the flanks of that chain, whether derived from the north or from itself. Of the *Tchornoi-Zem*, or black earth of the central regions of Russia, to which, quoting Baron A. de Meyendorf, M. de Beaumont refers in a long note, I will now only say, that having studied the nature and extent of this singular deposit over very wide regions, I intend, with the help of my fellow-travellers M. de Verneuil and Count Keyserling, to lay before the public very shortly a sketch of its relations to the northern drift and other superficial deposits of Europe.

the great surfaces of our continents were *immersed*, and not above the waters when by far the greater number of the phænomena on the surface of rocks was produced, I reject for those who entertain the same opinions as myself, the simple division into "glacialists" and "diluvialists," into which Dr. Buckland has divided the combatants on this question; for to whatever extent the former title has been won by Agassiz and himself, we who have contended for the submarine action of ice in former times, analogous to that which we believe is going on at present, can never be merged with those who, under the name of diluvialists, have contended for the rush of mighty waves and waters over continents. Besides glacialists and diluvialists, my friend must therefore permit me to call for a third class, the designation of which I leave to him, in which some of us desire to be enrolled who have advocated that modified view to which the general opinion is now tending.

The other point to which I allude, and bearing at once on this view, is a discovery which our Librarian has just made without quitting the apartments which he so truly adorns. In the American Journal of Science for the year 1826, Mr. Lonsdale has detected a short, clear, and modest statement, entitled "Remarks on Boulders, by Peter Dobson," which, though little more than one page in length, contains the essence of the modified glacial theory at which we have arrived after so much debate. First describing in a few lines the manner in which large boulders, weighing from ten cwt. to fifteen tons, were dug out in clay and gravel, when making the foundations for his own cotton factory at Vernon, and seeing that it was not uncommon to find them worn, abraded, and scratched on the lower side, "*as if done* (to use his own expression) *by their having been dragged over rocks and gravelly earth in one steady position*," he adds this most remarkable sentence:—"*I think we cannot account for these appearances, unless we call in the aid of ice as well as water, and that they have been worn by being suspended and carried in ice over rocks and earth under water.*" To show also that he had read much and thought deeply on this subject, Mr. Dobson quotes British authorities to prove, that as ice-floes constantly carry huge masses of stone, and deposit them at great distances from their original situation, so may they explain the transportation of foreign boulders to our continents.

Apologising therefore for having detained you long, and for having previously too much extended a similar mode of reasoning, I take leave of the glacial theory in congatulating American science in having possessed the original author of the best glacial theory, though his name had escaped notice; and in recommending to you the terse argument of Peter Dobson, a previous acquaintance with which might have saved volumes of disputation on both sides of the Atlantic.

In the mean time, however we may attempt to account for the transport of boulders, the striation and polish of rocks, and the accumulation of superficial detritus, we cannot quit the glacial subject without avowing our obligations to Venetz, Charpentier, and Agassiz, and above all to the last, for having brought the agency of ice more directly into consideration as a *vera causa*, to explain many phænomena on the surface. Even we who differ from Agassiz in his generalizations, and have not examined the Alps since the theory was propounded, should not hastily adopt opinions which may be modified after a study of the glaciers *in situ*. "Come and see" is the bold challenge of the Professor of Neuchatel to all who oppose him, and sanguine as to the correctness of his opinions, he is certain that many will be converted if they would but observe the phænomena on which his views are based. Truly we must acknowledge, that he was the first person who roused our attention to the effects produced by the bottom of an advancing glacier, and if geologists should eventually be led to believe, that certain parallel scratches and striæ on the rocks were in some instances due to glaciers moving *overland*, but in many other cases were produced by icebergs, we must remember that the fertile mind of Agassiz has afforded us the chief means of experimentally solving the problem.

In conclusion, Gentlemen, it is gratifying to reflect, that notwithstanding the vibrations of opinion which have been caused by the introduction of glacial action among geological dynamics, the fundamental principles of our science remain entirely unaffected. Conspicuous as it may appear through the attractive descriptions of Agassiz, or the eloquence of Buckland, the glacial theory must be considered an episode only in the records we are labouring to prepare of the grand changes of the planet. Let not, therefore, geology

be decried as a science without fixed principles, because her cultivators have recently differed upon a point which, though connected in theory with the science, has no bearing whatever on its uses nor upon the many fundamental points which it had previously established.

Your labours, Gentlemen, and those of your foreign associates, have already afforded proofs of the regular succession of the strata, and have traced their chronology; you have accurately marked the revolutions which have interrupted the sequence of by-gone races; you have explained the origin and position of various mineral substances essential to mankind, the dependence of geographical and agricultural products upon geological laws, and have shown how antagonist forces proceeding from the interior have modified the earth's outline, and been the cause of mineral wealth,—in a word, by your patient study of the masses you have acquired a true knowledge of the structure of the surface of the globe.

By these achievements the geologist has earned his best trophies, and has shown that the principles of his science are based upon the unerring laws of nature. Let then the shortness of his bright career incite us to renewed exertions, so that if at the close of life our vast subject should still present some unexplained phænomena, we may at all events have won the race in our own generation by establishing new landmarks in the rapidly increasing delta of natural knowledge.

THE END.

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